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Contributors:

MELISSA MERIAM BULLARD is an assistant professor of history at the University of North Carolina, Chapel Hill. She did her graduate work under Helmut Koenigsberger and received her Ph.D. in 1977. Her interest in family and marriage politics developed out of research for her forthcoming book on Filippo Strozzi and financial patronage under the Medici popes in the early sixteenth century.

DONALD R. KELLEY teaches history at the University of Rochester and has recently completed a book on the French Reformation. Ever since his *Foundations of Modern Historical Thought* (1970), he has been working on various aspects of the European legal tradition, including studies of a number of medieval and modern subjects—Baldus, Bodin, Vico, and Marx, among others. He plans to complete a larger work on jurisprudence in its relations to modern social and cultural thought.

JAMES F. POWERS, professor of history at the College of the Holy Cross, studied with Charles J. Bishko at the University of Virginia. The author's interest in the municipal military history of central medieval Castile and Aragón has led to articles in *Traditio* (1970) and *Speculum* (1971, 1977). He has re-

cently been awarded a grant by the Council for the International Exchange of Scholars to complete a monograph on Spanish medieval municipal militias and to study military themes in contemporary Romanesque and Gothic art. The municipal baths were originally studied as a check on legal evolution in the Spanish municipal charters, radically different as it was from military law, but the subject soon demonstrated its own unique possibilities.

HOWARD SPODEK, associate professor of history and urban studies at Temple University, received his training at Columbia University and the University of Chicago. Studies in Ahmedabad and Rajkot, India, supported by Fulbright and Danforth Fellowships and a National Science Foundation grant, have resulted in several articles and his *Urban-Rural Integration in Regional Development: A Case Study of Saurashtra, India, 1800-1960* (1976). Currently, Spodek is preparing a political biography of the nationalist politician, Sardar Vallabhbhai Patel, and a historiographical essay on urbanization in India. At Temple he is developing experiential modes of introducing foreign area studies into the curriculum.

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Gaius Noster: Substructures of Western Social Thought

DONALD R. KELLEY

“tu regere imperio populos, Romane, memento . . .”¹

GAIUS, THE MOST INFLUENTIAL OF CLASSICAL JURISTS, is apparently a modern discovery. The dramatic story of his resurrection has often been told. In 1816 the historian Barthold Georg Niebuhr came across a manuscript in Verona, a text from Saint Jerome written over a much earlier work. He reported this palimpsest (at some points *ter scriptus*, a double palimpsest) to his friend Friedrich Karl von Savigny, the greatest legal scholar of the age and the emerging leader of the so-called Historical School of Law. Savigny immediately recognized it as the work of the second-century jurist Gaius, otherwise preserved only in fragmentary form in that great sixth-century anthology, the *Digest* of Justinian. “Let us hope that we can decipher more of Gaius in these pages,” he wrote to Niebuhr. (So it fortunately turned out, for over eleven-twelfths of the *Institutes of Civil Law* were recovered with the help of this manuscript.) “This will surely give new life to our jurist,” Savigny predicted.² This book, the only nearly complete pre-Justinianian text, has indeed had a profound impact upon Roman legal history and scholarship since its publication in 1820.

Yet in a deeper sense Gaius had never really been lost to view. Despite the elementary and defective form in which his work was transmitted, the structure of his *Institutes* can be traced in the thought of many generations not only of jurists but also of historians and of political and social philosophers. Whether consisting of notes by Gaius himself or by a student (the same sort of debate that has raged around the works of Aristotle), the book became more influential than those of more authoritative jurists like Ulpian and Pomponius (who, unlike Gaius, possessed the right to interpret the law, the *jus respondendi*). Distilling, simplifying, and organizing the resources of ancient legal

As so often before, I must express gratitude to friends and colleagues at the Institute for Advanced Study for sympathetic stimulation and, for material support, to the National Endowment for the Humanities.

¹ Vergil, *Aeneid*, 6, 851. Also see note 85, below.

² Savigny to Niebuhr, December 5, 1816, in A. Stoll, *Karl Friedrich von Savigny*, 2 (Berlin, 1929): no. 321. Also see Savigny to Eduard Schrader, July 16, 1817, in *ibid.*, no. 33: “Das muss ein neues Leben unter unseren Juristen geben!”

wisdom, Gaius became the mentor not only of Rome but also of Byzantium and Europe. He was "the true architect of Justinian's collection," his most recent commentator has told us;³ he was the model for many later constructions as well, including national codes like the *Siete Partidas* and the *Code Napoléon*, legal treatises like Antoine Loisel's *Institutes of Customary Law*, Savigny's own *System of Modern Roman Law*, and a variety of more tangential philosophical ventures. Gaius' pedagogical role has been almost as various and substantial as that of Aristotle. To Justinian he was "Noster Gaius," and so he has remained for centuries to many others, including Savigny. He was, so to say, "our teacher."

"Gaianism" is not a conventional term, but in view of his role perhaps it should be. Many lesser authors—jurists as well as philosophers—have engendered eponymous "isms" with inferior claims, while Gaius formulated (if he did not create) one of the most distinctive and enduring systems of thought in Western history. It is not too much to say that Gaius established a dominant archetype of social thought, a more practical and human paradigm to rival the metaphysical structure of Aristotelianism.⁴ Though by intent a method of teaching law, his book suggested an epistemology, a potentially "scientific" method and various guiding principles for the systematic study of society and culture. His work exhibits, it seems to me, the fundamental kinship between the old science of jurisprudence (*legitima, legalis, or civilis scientia*, as jurists liked to call it) and the modern science of society.

In recent times, however, the Gaian tradition has been virtually unappreciated, and most likely unperceived, by historians of philosophy and of culture in general. What is the reason for this? Why has such neglect fallen on Gaius' *Institutes* and not, for instance, on Cicero's relatively vacuous *De Legibus*? One answer is surely that the study of law, even in Savigny's time, has diverged from history, literature, philosophy, and other fields with which it formerly had so many vital connections. A more important answer, perhaps, is that the significance of Gaianism—even for those disciplines apparently closest to jurisprudence, such as political and social philosophy—cannot be perceived merely from the literal and legalistic surface of the text. To appreciate this significance we must try to grasp the underlying meaning of the text by deciphering another—a historical and conceptual sort of—palimpsest. Not in any sense, however, should this approach be tried through speculative interpretation or what textual critics used to call "divination." Rather, we should proceed by trying to assess some of the implications that have in fact been

³ A. M. Honoré, *Gaius* (Oxford, 1962), 128. In general, see H. Wagner, *Studien zur allgemeinen Rechtslehre des Gaius* (Zutphen, 1978); and Pauly-Wissowa. RE 21, pt. 1 (1951): 286. The secondary (and tertiary) literature on Gaius and his text is enormous. The text, for example, is the subject of a periodical, *Gaius Studien*, begun in 1968, edited by R. G. Böhm, and published out of Freiburg. Here, as elsewhere, I will not attempt to provide bibliographical references on law and legal history, except as they bear upon relevant questions of social thought and, even then, only selectively in terms of the argument.

⁴ For the standard study, see F. X. Affolter, *Das römische Institutionen-System* (Berlin, 1897). Also see Helmut Coing, *Juristische Methodenlehre* (Berlin, 1972); and H. Jolowicz, *Roman Foundations of Modern Law* (Oxford, 1957), 61–81. For the most recent survey with an excellent bibliography, see G. Fassò, *Storia della filosofia del diritto*, 3 vols. (Bologna, 1966–70).

drawn out, some of the transformations that historically have been attempted: the aim is not to read between the lines of Gaius but, instead, to survey the career of Gaianism. The subject of these remarks is not *ille* but *noster* Gaius.

OF THE JURISCONSULT GAIUS LITTLE IS KNOWN, not even his full name, and his doctrinal background is not clear. When he began to teach and to assemble his book, there were already two schools of legal thought in Rome. One was the conservative and republican “Proclian,” the other the imperial-minded “Sabinian.” It may or may not be significant that Gaius belonged to the latter, more innovative group. Whatever its provenance, the power of Gaianism derives in general from three distinguishing features. The first is his basically historical approach, displayed most prominently in the fragment heading the famous second title of the *Digest*: “On the Origin of Law.”⁵ His historical orientation is, however, most concretely evident in the substance of the *Institutes*, which constitutes a kind of ordered cornucopia of Roman legal wisdom. The second is his dialectical method, which generated essential distinctions, divisions, and methods of interpretation. The last, and the most important, is the celebrated tripartite arrangement of social categories, a sequence of rubrics that entail not only moral priorities and a means of ordering reality but also a characteristic mode of perceiving and construing the world. What Gaianism suggests is not quite a doctrine (on the analogy, say, of Calvinism) but rather a methodological system (on the analogy of sixteenth-century “Ramism,” which accommodated Calvinism, yet was distinct from it), a framework for a *Weltanschauung* encompassing natural as well as social experience.

Although never satisfactorily explained, the Gaian triad, set forth under the rubric *De juris divisione*, has fascinated scholars for centuries. Down to the present day it has enjoyed, if not self-evident rationality, at least extraordinary authority in some circles.⁶ According to this anthropocentric, secular trinity, judicial and pedagogical cognizance had to be taken first of persons (*de personis*), second of things (*de rebus*), and last of actions (*de actionibus*): (1) “personality”—the *sine qua non*, so to speak—including degree of “liberty,” kinship, citizenship, and other social relations;⁷ (2) the “real” world, though for Gaius *res* could be intangible (*incorporales*) as well as tangible; and (3) the relations between and among subjects and objects—that is, “actions in a general sense, themselves divided reflexively into “personal” and “real” types. This presumably exhaustive classification represents, in effect, one enduring expression of the metaphysical foundations of social thought.

⁵ *Digest*, 1. 2. 1: “Gaius libro primo ad legem duodecim tabularum: Facturus legum vetustarum interpretatione necessario prius ab urbis initiis repetendum existimavi. . . .”

⁶ *Digest*, 1. 5. 3; and Gaius, *Institutiones*, 1. 8, in J. Baviera, ed., *Fontes Iuris Romani Antejustiniani* (Florence, 1940): “Omne autem ius quo utimur vel ad personas pertinet vel ad res vel ad actiones.”

⁷ In general, see Fritz Schulz, *Principles of Roman Law* (Oxford, 1936), 42, 140–63. And, in particular, see P. Duff, *Personality in Roman Private Law* (Cambridge, 1938); Paolo Zatti, *Persona giuridica e soggettività* (Padua, 1975); and Carlo Maiorca, *La Cosa in senso giuridico* (Turin, 1937). Also see Adolf Berger’s standard *Encyclopaedic Dictionary of Roman Law* (Philadelphia, 1953).

This distinctive form makes it possible to isolate a Gaian tradition, even though Gaius himself had no claim to originality or profundity. For underneath the Gaian text there are still older formulations. In fact, Gaius was not the first systematizer of Roman legal science. This honor seems to belong to Quintus Mucius Scaevola, a first-century B.C. jurist who, according to Fritz Schulz, created "the first dialectical system of law in the grand manner." The Mucian scheme was quadripartite, resting upon the divisions of inheritance law, persons, things, and obligations; but it bore obvious affinities to that of Gaius. As late as the second century Scaevola's work was the subject of commentaries by Pomponius and by Gaius himself. Thereafter, it disappeared and was unknown to Justinian's editors. "Well may we complain of a fate which has preserved so utterly worthless a work as Cicero's *De Legibus*," Schulz has written with characteristic assertiveness, "but which has allowed the book which laid the foundations of not merely of Roman, but of European, jurisprudence to perish."⁸ Paleographical fortune more than intellectual merit, then, leads us to celebrate a Gaian instead of a Mucian tradition, derivative and perhaps even degenerate as it might be in terms of Roman legal history.

What more fundamentally qualifies Gaian claims, of course, is the manifold indebtedness of Roman legal science in general to Greek philosophy and, more particularly, to Greek rhetoric. No one places much stock in the story (told by Pomponius and included in the *Digest*—paired, incidentally, with Gaius' celebration of history) about the visit of the *Decemviri* to Athens before the establishment of the law of the Twelve Tables, but the tale does nicely symbolize the cultural contribution of Greek thought. Although the channels of transmission are obscure, Platonic ideals, Aristotelian categories, sophistic *topoi*, and especially dialectical method constitute the original level, unfortunately illegible in detail, of the Gaian palimpsest. It was Greek conceptualization, especially the theory of interpretation so central to jurisprudence, that transformed the practice and teaching of Roman law into a Hellenistic "science" in the classical (as well as medieval and, before the present century, modern) sense of the term.⁹

More specific Greek influence can be detected at certain crucial points. Among the most important are the practice of making divisions (*divisio*, *differentia*, or, especially among medieval commentators, *distinctio*, corresponding to the Aristotelian *διαίρεσις*), the setting down of initial definitions (*definitio* or *regula*, corresponding to *ῥοι* or *κανόνες*), and a number of rhetorical topics, such as the contrast between the letter and the spirit, or meaning, of a statement (*verba* and *voluntas* or *ratio*). Indeed, many of the basic terms of civil law are simply translations from Greek terminology, such as the distinction between natural and civil law (*jus naturale* and *civile*, corresponding to

⁸ Schulz, *History of Roman Legal Science* (Oxford, 1953), 94. Also see G. Scherillo, "Il civilistico pre-Gaiano," in *Studi in onore di Vincenzo Arangio-Ruiz*, 3 (Naples, 1953): 445-67. On "abstraction," see Schulz, *Principles of Roman Law*, 53-65.

⁹ Schulz, *History of Roman Legal Science*, 62-69. In general, see the monumental work of Erich Wolf, *Griechisches Rechtsdenken*, 6 vols. (Frankfurt, 1950-).

[illegible]

Gaius, *Institutionum*, 4. 139-44, f. singulare.

φύσει δίκαιον and θέσει or νόμῳ δίκαιον), between the law of nations and civil law (*jus gentium* and *civile*, corresponding to κοινὸν δίκαιον and πολιτικὸν δίκαιον), and between written and unwritten law (*jus scriptum* and *non scriptum*, νόμος γεγραμμένος and νόμος ἄγραφος). So it was, too, with the idea of equity (*aequitas*, ἐπιείκεια) and the method of disputation (*disputatio in utramque partem*, δισσοὶ λόγοι).¹⁰

If Gaianism made use of Greek forms, it gave them a Latin dress and style. Indeed, behind the pedagogical intent of the *Institutes* there are philosophical implications that seem distinctively Roman. Consider, in the first place, the epistemological significance of the anthropocentric ordering of social categories. For Gaius understanding began not with cosmology, in other words, but with the human subject—the subject of consciousness, that is, as well as of rights. Thus, the problem of the human condition (*De condicione hominum* for Gaius, encompassed by the civilian rubric *De statu hominis*¹¹) provided the point of departure, and the philosophical implication was a sort of rudimentary sociology of knowledge. Having established the subject, horizons could be expanded to include other individuals and natural objects, potentially possessions; and, finally, the social field could be completed with consideration of actions and interactions of various sorts. Secondly, and correlative to this, the Gaian system was aimed not merely at causal explanation—value-free science, so to speak—but also at human problems and the rendering of practical judgments. On these grounds jurisprudence was useful as well as theoretical, concerned with the life active as well as the life contemplative, and enabled jurists to argue for the superiority of their “science” and to identify it with “true philosophy.”¹²

In a fundamental way, then, Gaianism was set apart from the naturalistic tradition of Greek philosophy. Perhaps the best way to clarify this is to recall the fundamental polarity that arose in Greek thought between nature (*φύσις*) and convention, or law (*νόμος*).¹³ “By convention exist color and taste,” said Democritus; “in reality there are atoms and the void.” As later elaborated by rhetoricians, Roman as well as Greek, the opposition was by implication between the world of human will and behavior (of symbols and “civilization”) and that of extra-human (objective or transcendent) reality. In the tradition of Roman law, of course, both concepts have their place: the first as a rational norm identified with natural law (*jus naturale*), the second as the human substance of civil law and that extra-Roman growth formulated as the law of nations (*jus gentium*). But the procedure was quite different. In contrast to the naturalistic or mathematical (especially Euclidean) strategy of starting with

¹⁰ See H. Hübner, “Subjektivismus in der Entwicklung des Privatrechts,” in *Festschrift für Max Kaser* (Munich, 1976), 5–42; and Giovanni Pugliese, “‘Res corporales,’ ‘res incorporales,’ e il problema del diritto soggettivo,” in *Studi in onore di Vincenzo Arangio-Ruiz*, 223–60. Also see the works cited in note 7, above.

¹¹ *Digest*, 1. 5; and Gaius, *Institutiones*, 1. 3.

¹² For this related subject, see my “Vera Philosophia: The Philosophical Significance of Renaissance Jurisprudence,” *Journal of the History of Philosophy*, 14 (1976): 267–79, and the references cited therein.

¹³ F. Heinemann, *Nomos und Physis* (Basel, 1945); and Wolf, *Griechisches Rechtsdenken*, *passim*. Also see C. A. Maschi, *La Coscienza naturalistica del diritto et degli istituti giuridici romani* (Milan, 1937); and F. Lanfranchi, *Il Diritto nei retori romani* (Milan, 1938).

general principles and reasoning from them, Gaianism gave prominence and priority to the human aspect, to what from the twelfth century onward would be termed “positive law,”¹⁴ and then advanced, empirically in a sense, to higher degrees of rationality and universality.

What about the structural significance of Gaianism? Does the secular trinity of persons-things-actions reflect some religious or procedural convention? Does it have some logical or linguistic base? Or is it merely, as one modern expert has suggested, one more manifestation of a general Roman fixation on “trichotomy”?¹⁵ Whatever the prehistorical source, the arrangement was repeated and elaborated in countless ways for many centuries down to the present day. It antedated, and became hardly less ingrained than, the Christian trinity; and, because of its function in liberal as well as legal education, it may have played a larger role in the history of thought than has generally been recognized. Almost seventeen centuries after Gaius the young Karl Marx complained about the trichotomizing (*trichotomisch*) method forced on him by Savigny and other of his teachers—but not before Marx composed a three-hundred-page treatise of private law based on the Gaian scheme.¹⁶ And, though he abandoned both this scheme and the “idealism” associated with it, Marx went on, under Hegel’s influence, to adopt another not dissimilar trichotomy likewise based on a dialectic of subject-object. In other ways, too, ghosts from the Roman past haunted Marx’s mature works, but of course this discussion goes beyond the confines of the direct tradition of Gaianism.

AFTER GAIUS’ DEATH (A.D. 178 OR LATER) his authority grew in legal as well as pedagogical terms, especially from 426 onward, when the law of citations named him as one of five jurists to be followed in the courts.¹⁷ The worldwide appeal of his *Institutes* is suggested by various survivals outside Italy, such as the excerpts made for the *Breviarium* of Alaric and the sixth-century commentary made for the law school of Autun. At about the same time, Justinian remarked that Gaius’ work was the only elementary textbook assigned to law students in the Byzantine schools, though by 533 it was already obsolete in some respects.¹⁸ In fact, Justinian, or rather his editor Tribonian, preserved Gaius for posterity, until the nineteenth century at any rate, not only by citing him often (531 times to be exact) in the *Digest* but also by making his book the model for Justinian’s own *Institutes*. In his preface the emperor revealed his philosophic intention of “bringing into bright harmony” (*in luculentam ereximus*

¹⁴ Steven Kuttner has shown that “positive law” (*jus positivum*) comes from a misreading of “posited law” (*jus positum*); Kuttner, “Sur les origines du terme ‘droit positif.’” *Revue historique du droit français et étranger*, 15 (1936): 728–30.

¹⁵ H. Goudy, *Trichotomy in Roman Law* (Oxford, 1910); and Karl Friedrich von Savigny, *System des heutigen römischen Rechts*, 1 (Berlin, 1940): 322–26.

¹⁶ Marx to Heinrich Marx, November 10–11, 1837, in *Marx-Engels Gesamtausgabe*, 1, pt. 1 (Berlin, 1927): 213–31; and see note 87, below.

¹⁷ *Codex Theodosianus*, 1. 4. 3.

¹⁸ “Deo auctore,” Constitution prefacing the *Digest*

consonantiam) the confused mass of ancient jurisprudence and legislation, and, indeed, just at this point he made the famous proprietary claim that his manual was largely based on the work of “our Gaius” (*praecipue ex commentariis Gaii nostri. . .*).¹⁹ Through this Byzantine vehicle and, secondarily, through the *Digest* the form and substance of Gaianism was transmitted to modern European culture.

The *Institutes* of Justinian began with the same premise as the *Institutes* of Gaius: “All of our law is related either to persons or to things or to actions.” Justinian then explained the reason for that order—“it is of little purpose to know the law if we do not know the persons for whom the law was made.”²⁰ Accordingly, Book I is a topical discussion of the title, “De personis,” including analyses of the levels and limitations of personal liberty, especially in terms of paternal and marital power (the *patria potestas* of the father and the *potestas de manu* of husband and wife). Books II and III continued “De rebus” and were devoted to particular problems of possession and property, exchange and inheritance, except for those things designated as public, common to all, or unpossessable because of their ecclesiastical character (*res sacrae*). Finally, Book IV treated “De actionibus”—logically and interlockingly divided into personal and real things—and concluded with judicial procedure and criminal actions.

This Byzantine effort at remodeling differs in two fundamental respects from the Gaian original. One difference is the considerable emphasis placed upon public law, illustrated above all in the notorious, post-Gaian text from Ulpian, “What pleases the prince has the force of law” (*Quod principi placuit legis habet vigorem*).²¹ This emphasis, of course, arose from Justinian’s determination to exalt, if not to deify, his imperial authority above all previously recognized sources of law, including juristic responses (*responsa prudentum*); but it did not directly affect the social categories encompassed by private law. The other difference is that Justinian, drawing on later classical jurisprudence and rhetorical hyperbole, made larger philosophical claims for the science of law, even claiming for it the status of true wisdom (*divinarum ac humanarum notitia*, the equivalent of the Ciceronian formulation of *sapientia*). But his hyperbole only served to enhance the attractions of Gaianism.²² There is no denying that from this point most of the words of the Roman legal canon are those of Justinian (or rather his paraphrase of many generations of jurists), but in many respects the assumptions and conceptual base continue to be those of “our Gaius.” In any case, in this somewhat disguised form Gaianism was passed on to the West and preserved for the next phase of its posthumous career.

¹⁹ “Imperatoriam maiestatem,” Constitution prefacing the *Institutes*. And, in general, see L. Wenger, *Die Quellen des römischen Rechts* (Vienna, 1953), 600–37.

²⁰ *Institutes*, I. 2. 12. Also see Gaius, *Institutiones*, I. 8.

²¹ *Institutes*, 7. 21. 6.

²² *Digest*, I. 1. 1. The system of the *Digest*—and, indeed, that of the *Code*—has its own character and history; but it is closely related to, and in some ways dependent on, that of the *Institutes*, and it was much more severely criticized in later centuries. Because of its pedagogical and “methodical” purpose and because of its broader impact, the *Institutes* has been chosen for examination here.

The third level of the Gaian palimpsest, historically *ter scriptus*, began during the twelfth-century revival of Roman law and the establishment of professional faculties of law at the University of Bologna and elsewhere.²³ At this stage the problem of deciphering becomes increasingly complicated by the intrusion of other intellectual forces, including canon and feudal law and Aristotelian philosophy in its scholastic form. Henceforth, the Gaian tradition was enmeshed in glosses, commentaries, questions, opinions, and—though strictly forbidden by Justinian himself—more or less distorting “interpretations.”²⁴ As Gaius’ work was lost, so Gaianism became increasingly difficult to perceive.

Nevertheless, the basic principles were preserved, and certainly no alternatives were suggested. The most authoritative of glosses, that by Accursius, remains mostly grammatical corrections, small quibbles, and large justifications. About the first Gaian text in the *Digest*—the law “omnes populi” declaring that “all people are ruled by civil law or the law of nations”—Accursius wondered, “But what of those who are not so ruled?” The answer was not difficult: “I respond that these are uncivilized men.”²⁵ This simplified version of the *disputatio in utramque partem* Accursius also applied to the broader question whether Gaius’ threefold division might be false (*hoc videtur esse falsum*), and again he resolved the difficulty, in this case by arguing for a more than merely literal view of “things” and “actions.” There was also a tendency, illustrated by Petrus de Bellapertica, to replace “action” by the more sophisticated concept of “obligation,” which arose from an agreement (*obligatio nascens ex contractu*) and was guaranteed by natural law.²⁶ But despite such rationalizing tendencies the Gaian system was not seriously questioned.

Following the Glossators from the late thirteenth century, the still more philosophically minded Commentators began to diverge more basically from Gaian principles. Concerning the same law “Omnes populi,” for example, Baldus had a different answer to the problem whether “all people” were indeed ruled as Gaius had asserted. “I respond that not all are,” said Baldus, “for many cities have their own statutes by which they are ruled.”²⁷ Although technically the emperor was still the only source of law, the right of people to make their own law (*jus proprium* or *jus propriissimum*) could be justified in legal

²³ For general orientation on the Roman legal tradition, see Paul Koschaker, *Europa und das römische Recht* (3d ed., Berlin, 1958). And, for bibliography, see Franz Wieacker, *Privatrechtsgeschichte der Neuzeit* (2d ed., Göttingen, 1967); and Walter Ullmann, *Law and Politics in the Middle Ages* (Ithaca, N.Y., 1975). On particular themes, see E. Cortese, *La Norma giuridica*, 2 vols. (Milan, 1962–64); and my “Clio and the Lawyers: Forms of Historical Consciousness in Medieval Jurisprudence,” *Medievalia et Humanistica*, new ser., 5 (1974): 25–29.

²⁴ M. Fuhrman, “Interpretatio,” in *Symptotica* Franz Wieacker (Göttingen, 1970), 80–110; F. Pringelsheim, “Justinian’s Prohibition of Commentaries to the Digest,” in his *Gesammelte Abhandlungen*, 2 (Heidelberg, 1961): 86–106; and V. Piano Mortari, “Il Problema dell’interpretatio nei commentatori,” *Annali di storia del diritto*, 2 (1958): 29–109.

²⁵ Accursius, *Ad Digestum*, 1. 5. 1, which is the equivalent of Gaius, *Institutiones*, 1. 8.

²⁶ Petrus de Bellapertica, *In libros Institutionum commentarii* (Lyon, 1536), 111. And, in general, see C. Karsten, *Die Lehre von Verträge bei den italienischen Juristen des Mittelalters* (Rostock, 1882).

²⁷ Baldus, *Super Digesto veteri* (Venice, 1535), f. 26. In general, see *L’Opera di Baldo*, per cura dell’Università di Perugia nel V centenario della morte del grande giureconsulto (Perugia, 1901); and, in particular, see J. Tarducci’s essay in *ibid.*, 409–66; Norbert Horn, *Aequitas in den Lehren des Baldus* (Cologne, 1968); and, for the classic work, C. N. S. Woolf, *Bartolus of Sassoferrato* (Cambridge, 1913).

terms, Baldus argued; for did not the imperial rule against a people making "illicit statutes" imply that they could make *licit* ones? In this way Baldus, following his master Bartolus and other Italian jurists, built up the case for the legitimacy of city-states and other political units that were independent of imperial jurisdiction and so, presumably, of Roman forms. For some jurists, of course, there was an answer to this problem, too: such post-Roman statutes and customs should be included in the *jus gentium*.

Gaius' famous statement about the legal "condition of men" likewise received expanding interpretation—and "interpretations," as Accursius reminded his students, implied correction. In the first place the topic "De statu hominum" was an invitation to some jurists to philosophize about the human condition in general, about the natural as well as the social state of that "most worthy of creatures" (*dignissima creaturarum*), as Alberigo de Rosate called man. But the main theme was the historical change that had occurred since the time of Gaius, the difference between the ancient and the "modern state of men" (*status modernus hominum*). "In modern times" (*secundum moderna tempora*), observed Alberigo, "there are various states of men that are not treated under the title" of Gaius. "And, because the state of men is in constant motion and never at rest," he proposed to bring the discussion up to date by treating a variety of more recent questions, including those of the Jews and Saracens and different sorts of Christians (real and supposed)—laymen and clerics.²⁸ In fact, the general problem that absorbed most of the energies of European jurists was how to accommodate their own experience and institutions, feudal as well as ecclesiastical, to Roman forms and rules or, in other words, how to transform, as Justinian had transformed, the *jus antiquum* into a *usus modernus*. In this effort jurists increasingly turned to extra-Roman experience, but they did so in orthodox terms of custom (*consuetudo*) and the *jus gentium*, based (as Gaius had said) on "natural reason."

Another major force that acted to transform Gaianism was the intrusion, or rather re-intrusion, of Aristotelian philosophy. Attempts to find or to impose logical species, especially the use of the famous "four causes," again illustrates the infiltration of naturalism and metaphysics into jurisprudence and social thought. Accursius had accepted Gaius' demand for the study of the "origin of law," though he interpreted "necessary" as "useful" and justified it simply on grounds of logic and propriety—that it provided a way of introducing basic elements (*principia*) and that it agreed with the method of a lawyer (*advocatus*), which was to start with his exordium. For commentators like Baldus, however, concern for origins and sources was translated into the system of Aristotle's efficient, material, formal, and final causes; and, indeed, this procedure was associated with the growing claims of scholastic jurisprudence to be a rational "science" because it was universal and treated its material in terms of cause and effect (*per causas*).²⁹ The long-range implica-

²⁸ Alberigo, *In primam FF [Digesti] Veter. part.* (Venice, 1585), f. 44v.

²⁹ See R. Weigand, *Die Naturrechtslehre der Legisten und Dekretisten von Irnerius bis Accursius und von Gratian bis Johannes Teutonicus* (Munich, 1967); and my "Vera Philosophia: The Philosophical Significance of Renaissance Jurisprudence," 274.

tions of these tendencies were momentous; they continued to inform professional jurisprudence down to Savigny's time and after. Yet Gaianism was modified rather than replaced; another level was added to the multilayered Gaian text.

SERIOUS CRITICISM AND TRANSFORMATION of the Gaian paradigm began in the sixteenth century. The first condition of this mutation was the rise of humanistic jurisprudence, beginning in fifteenth-century literary circles and then infiltrating the academic and professional study of law.³⁰ From the second quarter of the century deepening historical perspectives and expanding geographical horizons encouraged by humanism led certain jurists to begin the task of "reforming" the Roman system of jurisprudence fixed by Gaius, codified by Justinian, and canonized by the medieval Glossators. The "reformers" wanted to fulfill the famous Ciceronian ideal of "reducing law to an art" (*jus in artem redigendo*)—or, better, Justinian's ideal of elevating it to a science, a concept that was vital and a prime rubric of Romanist jurisprudence. Pursued by elaborate violations of Justinian's strictures against "interpretation," the task was justified in the name of that abstract reason contained in the law (*recta ratio*; *ratio juris*) as well as a novel concern for "method," which was a dominant force in the intellectual life of the sixteenth century. The men involved in this enterprise were the founders of that modern school of jurisprudence that Savigny designated "the systematists" (*die Systematiker*) and that represents a latter-day Gaianism—the last, or perhaps next-to-last, layer of the Gaian palimpsest.³¹

Underlying this tradition was another movement for "reform" associated with humanism, namely the attempt to improve and to re-order dialectic by bringing it into contact with, or even submerging it in, rhetoric.³² This aim of various pedagogical reformers, including Rudolf Agricola and Petrus Ramus, was carried over by like-minded jurists into their own field of study, which, of course, had other longer-standing ties with rhetoric in technical ways. In 1520, building on Agricola's work, Claudius Cantiuncula published his *Topica legalia* in an effort to find a better arrangement for the major "commonplaces"

³⁰ On the historical implications of "legal humanism," see D. Maffei, *Gli Inizi dell'umanesimo giuridico* (Milan, 1956); and my *Foundations of Modern Historical Scholarship* (New York, 1970). For the philosophical implications, see Hans Troje, *Graeca Leguntur* (Cologne, 1971), which is useful for orientation, new insights, and bibliography if not for all of its judgments.

³¹ Savigny, *Das Recht des Besitzes* (7th ed., Vienna, 1865), 10. For the classic and still indispensable work on this tradition, see R. Stintzing and E. Landsberg, *Geschichte der deutschen Rechtswissenschaft*, 4 vols. (Munich, 1880–1910).

³² See, in general, Neal Gilbert, *Renaissance Concepts of Method* (New York, 1956); and Walter Ong, *Ramus, Method, and the Decay of Dialogue* (Cambridge, 1958). On law in particular, there has been an upsurge of work lately; see, for example, Hans Troje, "Wissenschaft und System in der Jurisprudenz des 16. Jahrhunderts," in J. Blühdorn and J. Ritter, eds., *Philosophie und Rechtswissenschaft* (Frankfurt, 1969), 63–88; Aldo Mazzacane, *Scienza, logica, e ideologia nella giurisprudenza tedesca del sec. XVI* (Milan, 1971); F. Carpintero, "'Mos italicus,' 'mos gallicus,' y el humanismo racionalista," *Ius Commune*, 6 (1977): 108–71; C. Vasoli, "La Dialettica umanistica e la metodologia giuridica nel secolo XVI," in *La Formazione storica del diritto moderna in Europa*, 1 (Florence, 1977): 237–79; and A. Giuliani, "The Influence of Rhetoric on the Law of Evidence and Pleading," *Juridical Review* (1962), 216–51.

of law (*locorum divisio*).³³ This work was pursued further in the *Methodica dialecticis ratio* by Johann Apel, who illustrated his topical reform with a series of diagrams showing the logic of social, especially as contrasted to natural, structure and process. Among German jurists like Johann Frey and Nicolaus Vigelius the influence of Ramus was particularly strong, partly because they hoped with his help to restore jurisprudence to a more central position in general learning and, through its association with oratory, to enhance its social utility. As Frey suggested in his schematic prescription for an “idea of the good and complete jurisconsult,” the finished product, a paradigm of practical and theoretical knowledge, would be the “political man” (*homo politicus*).³⁴ But most of all, raising their sights from pedagogy to philosophy, these jurists wanted to fashion the Roman legal tradition into an orderly system; for “order,” wrote Christopher Ehem, another of these legal reformers, “is the soul of things themselves” (*ordinem esse animam ipsarum rerum*).³⁵

Although not generally appreciated or even admitted, the movements to reform dialectic and jurisprudence continued to have recourse to old traditions of naturalism and scholasticism, despite a lot of careless anti-Aristotelian rhetoric. Johann Oldendorp, in another treatise on “legal topics,” used Aristotelian categories to organize his *materia* and *exempla* and the idea of the four causes to interpret “actions” in particular.³⁶ Even more systematically, Vigelius, who indeed set out to reorganize in such dialectical terms the whole *Institutes* of Justinian (*resolutio Institutionum Imp. Justiniani*), relied upon Aristotle.³⁷ Similar attitudes are evident in contemporary theories of “interpretation,” which, despite Justinian’s prohibition, had become a recognized part of law.³⁸ The “order” sought by critical methodologists like Jean Coras and Pietro Gammaro was a natural order, which they contrasted sharply with civil law. The latter was founded “not on nature,” as Coras observed, “but only on opinion and the authority of legislators.”³⁹ This social authority might suffice for law as an “art,” but a science needed to judge “through causes”—precisely what the new theories of interpretation proposed to accomplish. A rational method, or methodical rationale (*methodica ratio*), as

³³ Cantiuncula listed twenty-five places or means of making distinctions; see his *Topica legalis*, printed with Johann Apel, *Methodica dialecticis ratio ad iurisprudentiam* (Basel, 1545).

³⁴ Nicholas Reusner, ed., ΧΕΙΡΑΓΩΓΙΑ sive *Cynosura iuris*, 2 (Speier, 1588), no 32; Freigius, “Idea boni et perfecti iuris consulti” (fold-out diagram in the appendix). Also see Mazzacane, *Scienza, logica, e ideologia*.

³⁵ Ehem, *De principis iuris libri septem* (Basel, 1556), 1.

³⁶ Oldendorp, *Actionum forensium progymnasmata in septem classes distincta* (Lyon, 1566), f. 3v. Also see his *Topica legalis* (Lyon, 1555). On Johann Oldendorp, see E. Wolf, *Grosse Rechtsdenker der deutschen Geistesgeschichte* (2d ed., Tübingen, 1951), 134–75; and Guido Kisch, *Erasmus und die Jurisprudenz seiner Zeit* (Basel, 1960), 227–59; *Claudius Cantiuncula* (Basel, 1970), 57–71 (“Methodenlehre”); and *Melanchthons Rechts- und Soziallehre* (Basel, 1967). For a topical analysis of Bartolus, see A. Brederode, *Loci communes . . . novi et uberrimi in Bartoli . . . opera omnia* (Basel, 1589).

³⁷ Vigelius, *Dialectici iuris civilis libri III* (Basel, 1620). Also see his *Methodus universi iuris civilis absolutissima* (Lyon, 1591).

³⁸ See V. Piano Mortari, *Ricerca sulla teoria dell’interpretazione del diritto nel sec. XVI* (Milan, 1956). Also see H. Schürpf, “Ratio interpretandi,” in Stintzing and Landsberg, *Geschichte der deutschen Rechtswissenschaft*, 1: 1071.

³⁹ Coras, *De iure civile in artem redigendo*, in *Tractatus universi iuris*, 1 (Venice, 1584): 59; and Gammaro, *De extensionibus*, in *ibid.*, 18: 247. On Coras, see A. Fell, “The Classical Four Causes in the Renaissance Art of Law” (Ph. D. dissertation, Columbia University, 1974).

Oldendorp put it, “requires that the interpreter of law have recourse to the law of nature.”⁴⁰

Yet this widespread search for system—“methodus,” “schemata,” “ratio,” and “partitiones” are among the operative terms—by no means implied abandoning the human and positive aspects of jurisprudence. The jurist had to judge fact as well as law; he had to find authorities as well as devise arguments. The dual aim of jurisprudence, according to Conrad Lagus, was to answer both the historical and the philosophical questions: Philosophy, he explained, was no doubt “the first part of law, that is, the truth and reason of law insofar as the human mind can attain them”; but no less essential was “the second part” (that is, history), “the bare narration of facts . . . to show the forms of law observed by Roman legislation in particular cases.”⁴¹ Precisely in this sense of “history” could law be analyzed in terms of persons, things, and actions. In the preface to his standard anthology of legal treatises on method, Nicolas Reusner provided the aphorism that best expresses the enduring importance of the Gaian scheme: “Bonus Institutionalista,” he stated, “bonus Jurista.”⁴²

The continuing and cumulative critique of the Roman canon was an international enterprise; but the major efforts of revision were begun in France, especially by disciples of Andrea Alciato at the University of Bourges. Alciato was the founder of humanistic jurisprudence, of the *mos gallicus* as it was later called, although he personally lacked any particular systematic interests. The most notable of his followers—François Connan, Eguinaire Baron, François le Douaren, and Hugues Doneau—were deeply indebted to the humanist movement; but each made a practice of also introducing modern materials and ideas, not only feudal law, which was regularly taught at Bourges, but a variety of historical issues, such as the influence of Celtic and Frankish institutions. Each of them also contributed massively to the criticism of the Roman legal canon, and none hesitated to break with Roman forms in their attempts to realize the ideal of law as “true philosophy.”

Connan’s master work, published posthumously in 1557, was called *Commentaries on Civil Law*; but its general purpose was actually to shift emphasis away from civil law and to discuss instead such topics as obligation and property in terms of the law of nations, as Gaius had defined it. For Connan the *jus gentium* was equivalent to the “form of a people” (*forma populi*), and it was, logically if not chronologically, “prior to civil law.” As a consequence, he tended to examine conventional topics of civil law in the context not only of natural law, identified with the “first law of nations” (*ius hoc gentium primum*), but also of European, often comparative, history. He also gave large and continuing roles to custom in the progress of legal institutions and to jurists in rationalizing these customs. “Custom interprets law,” he wrote, and “judgments confirm custom” (*consuetudo legum interpretatur; res judicatae consuetudinem*

⁴⁰ Oldendorp, *Iuris naturalis gentium et civilis εἰσαγωγή* (Antwerp, 1539), sig. Aiii.

⁴¹ Lagus, *Methodus iuris utriusque traditio* (Lyon, 1566), 3.

⁴² Reusner, *ΧΕΙΡΑΓΩΓΙΑ sive Cynosura iuris*, 1: 13.

confirmat).⁴³ Law was, of course, grounded in nature, but the first laws (*priscæ leges*) were barbaric; there was no original “golden age” (*aetas aurea*), only “the rule of lusts, factions, evils, seditions, plunderings, war, and an absence of equity and justice.”⁴⁴ The perfection of law only developed over the course of time, with the ratiocinations of jurists and especially with the efforts to establish a rational system of law. Connan had reservations about the triadic system of Gaius and argued that the rubric of actions was unnecessary since it could be divided between persons and things. On a more general level, however, Connan carried on the spirit of the Gaian tradition.

So, in even more modernizing and vernacularizing terms, did Eguinaire Baron, the eldest and in some ways the most original of these four French pioneers. In addition to a manifesto of the new jurisprudence, Baron produced a series of commentaries on all parts of civil law according to a unique and self-consciously comparative method that he termed *commentaria bipertita*, by which he “accommodated civil law to French customs and legislation” (*accommodata hujus tituli ad mores Galliarum et leges regias*).⁴⁵ He “accommodated” both the *Institutes* of Gaius and the *Digest* of Justinian, in part simply by translation. He explained, for example, that “among the French the *plebs* is the Third Estate” and went on to point out parallels and contrasts in the respective social and legal systems. He agreed with Gaius that all law concerned persons, things, or actions, but his experience prompted him to expand “the condition of man” beyond the distinction of free and unfree. Women were “persons” too, and, like Connan, he was aware of their peculiar position (*quia status foeminarum deterior in multis causis*). There were also the problems of foreigners and naturalization (*aulbains, ou étrangers, et lettres de naturalité*); and, in fact, Baron composed a monograph on succession among foreigners.⁴⁶ Other difficulties, such as the antiquated Roman paternal power (*patria potestas*), likewise led him to depart from Roman convention, though without rejecting the entire Roman framework of jurisprudence. The work of Baron, a marvelous and many-sided illustration of the cultural shock that antiquity could inflict on sensitive and learned moderns, certainly deserves closer study.

Better known and probably more influential is the work of Baron’s colleague and rival Le Douaren. Although these two “Alciateans” had a variety of disputes, personal and religious as well as professional and political, they fundamentally agreed about “method”; and Baron subscribed to his younger colleague’s formulation that emphasized the systematic-pedagogical (*ars docendi*) as well as the humanistic-philological (*sermonis proprietas*) aspects of the

⁴³ Connan, *Commentariorum iuris civilis libri X* (Paris, 1557), f. 43. For analyses, see C. Bergfeld, *Franciscus Connanus* (Graz, 1968); and V. Piano Mortari, “La sistematica come ideale umanistico dell’opera di Francisco Connano,” in *La Storia del diritto nel quadro delle scienze storiche* (Florence, 1966), 521–31. The same formula appears in Lagus’ *Methodus iuris utriusque traditio*, 42, from the *Digest*, 1. 3. 37.

⁴⁴ Connan, *Commentariorum iuris civilis libri X*, ff. 32v, 16.

⁴⁵ Baron, *Opera omnia*, ed. F. Baudouin (Paris, 1562), *Commentarii*, *passim*, and *Institutionum civilium ab Iustiniano Caesare editarum libri IIII* (Poitou, 1550). On Baron there is no useful study of any sort.

⁴⁶ Baron, *Opera omnia*, 1: 52, 78 (“De statu hominum”), 92.

new jurisprudence.⁴⁷ For Le Douaren legal education ought to begin with the *Institutes* and, despite the humanist prejudice against scholasticism, ought to include the works of Bartolus, Baldus, and the best of the philosophical commentators. Like his friend Connan, Le Douaren was devoted to the ideal of “true philosophy,” and he declared that “there is nothing either divine or human that the jurisconsult does not treat and that is not relevant to civil science.” He wrote extensively on both canon law and, under the civilian heading of “custom,” feudal law; and he denied that Roman law had in any sense been “received” into France. Nevertheless, civil law continued to provide the terms and framework of his legal philosophy. In his discussion of the problems of legal system, he leaned perhaps more toward the naturalistic interpretation, making an analogy to mathematics in particular, since he believed that “the elements of law, the grounds of its maxims and basic issues are as points, lines, surfaces, etc., are to geometry.”⁴⁸ Such was the basis of Le Douaren’s various “methodical” discussions (*methodica enarratio*, *methodica partitio*, and *tractatio*, as he characterized his interpretations of the Roman canon).

Of all of the work of the great “systematists,” that of Hugues Doneau, disciple and successor of Le Douaren at Bourges, is probably the most comprehensive and influential. For Savigny, Doneau’s *Commentaries on Civil Law* marked the introduction of the French school into Germany, and for Savigny’s disciple R. Stintzing, the great authority in the field, Doneau was “the creator of modern jurisprudence.”⁴⁹ At Bourges and later at the Universities of Altdorf and Heidelberg, Doneau carried on the work of transforming Romano-Byzantine law into a universal system in accordance with the ideal of law as “true philosophy,” as an expression of “right reason.”⁵⁰ Like many other contemporary “anti-Tribonianists,” Doneau was much disturbed by the state of the *Code* and especially of the *Digest* of Justinian, although it was the form, or rather the formlessness, instead of the substance (the post-classical interpolations, or “Tribonianisms”) that he wanted to correct. The *Institutes*, on the other hand, he continued to admire, especially the tripartite classification; and, in fact, much of his work of “reformation” consisted of trying to gather together and to arrange the scattered and sometimes contradictory material on persons, things, and actions into an intelligible and symmetrical system. For Doneau each of these headings had three sub-headings: (1) divine and human, with the latter divided into public and private; (2) obligations, which followed the discussion of rights and which were grouped under “things”; and (3) forms of procedure to obtain rights, which appeared under “actions.” Doneau’s work, like that of his elder colleagues, shows the strong imprint of formal philosophy, including the four

⁴⁷ Le Douaren, *Opera omnia*, 1: 1. For an analysis, see W. Vogt, *Franciscus Duarenus* (Stuttgart, 1971).

⁴⁸ Le Douaren, *Opera omnia*, 1: 1. Also see Coras, *Opera omnia*, 1 (Wittenberg, 1603): 1.

⁴⁹ Stintzing, *Hugo Donellus in Altdorf* (Erlangen, 1869), 42.

⁵⁰ Doneau, *Opera omnia*, 1 (Rome, 1828): 133. On Doneau, see the old appreciation and analysis by A. Eyssell, *Doneau* (Dijon, 1860); on Doneau’s significance for French systematizing and codifying efforts, see A.-J. Arnaud, *Les Origines doctrinales du Code civil français* (Paris, 1969), 121.

Aristotelian causes, and at least a touch of Ramist, or Ramoid, method; but in general the *Commentaries* remains within the Gaian tradition.

In the generation after Doneau the rising tide of “vernacular humanism” brought this tradition into contact with the tangled question of customary law. In France two close colleagues who studied at Bourges, Étienne Pasquier and Antoine Loisel, took Justinian’s *Institutes* as the model and guide for their efforts to bring some system to vernacular law. Pasquier found parallels to the Gaian model in other “institutes”—Quintilian’s “oratorical,” Lactantius’s “divine,” and Ramus’s “dialectical” (to which we might add Calvin’s “institutes” of religion)—and began his work with a short history of Roman systematizing from the time of Scaevola.⁵¹ Pasquier argued that law was more closely allied to rhetoric than to philosophy and that, because of the jurist’s reliance on memory and human judgment, his art was called *juris prudentia* instead of *juris scientia*. Like his Latinate colleagues, Pasquier operated in the domain of the *jus gentium*, which he equated simply with “human law” (*droit humain*). His *Interpretation of the Institutes of Justinian* (unpublished until the nineteenth century) started out as a translation but ended up as a comparative study of Roman and French legal institutions, following the Gaian pattern very much in the style of Baron’s work. Pasquier, however, was generally hostile to civil law, deplored its tyrannical and rigid tendencies, and was at pains to point out contrasts with French customs. With respect to the first division, for example, all French “persons” were free (*libres et franchises*) except in a few antiquated provincial *coutumiers*. In most ways Loisel agreed with and, indeed, consulted Pasquier on these matters, and was more insistent on the differing status of French “persons,” arguing that the primary division was between *noble* and *roturier* (itself divided into *bourgeois* and *vilain*), since all men were or could become free by baptism. Loisel’s *Customary Institutes*, again following the Gaian pattern but composed of native proverbs, literary expressions of folk wisdom, and maxims of customary law, moved even further from Romanism toward some sort of national system.⁵²

There were other efforts at system-building in the sixteenth century, but these works, like the celebrated *Republic* of Jean Bodin, tended to be concerned with political rather than social or legal thought. One instructive exception, abused when not neglected, is the curious construction fashioned by Bodin’s rival, Pierre Grégoire of Toulouse. For Grégoire “method” was an “imitation of nature,” and in his own grandiose *Republic* he tried to assemble a man-centered cosmology to emulate the natural cosmos.⁵³ Grégoire, like Bodin, denied the universality of the Roman tradition and, specifically, French subjection to it; but on the fundamental point, the nature of the commonwealth, he had a very different emphasis. The center of his political cosmos was occupied not

⁵¹ Pasquier, *L’Interprétation des Institutes de Justinian, avec la conference de chaque paragraphe aux ordonnances royaux, arrestz de Parlement et coustumes générales de la France*, ed. M. le duc Pasquier (Paris, 1847), 9, 45.

⁵² Loisel, *Institutes coutumiers*, ed. M. Reulos (Paris, 1935).

⁵³ Grégoire, *De republica libri sex et viginti* (Frankfurt, 1609), 10: 54, 13: 12, 21. And see the discussion by C. Collot, *L’École doctrinale de droit public de Pont-à-Mousson* (Paris, 1965).

by the prince—as it was for Bodin and for that “most pernicious man, Machiavelli”—but by that Roman form of wisdom called law. This emphasis is even clearer in Grégoire’s other systematic effort, his *Syntagma juris universi*, a book treating “all law, divine, human, and natural,” according to a “new method.”⁵⁴ The novelty of this method, however, fades on examination, not only because it relies on traditional views of hierarchy but also because its principal categories turn out to be variations on the old Gaian theme. The first volume (books 1–6) covers a wide range of things—natural, divine, and human (including “communal” and “feudal”)—and then (books 7–19) persons and the various conditions thereof. The second volume concludes with an elaborate consideration of human “actions,” criminal as well as legitimate, collective as well as individual, public as well as private. Reversing the position of the first two categories was done for the orthodox reason that in Creation things had in fact preceded persons. In general, Grégoire’s work illustrates a crucial turning point in the career of Gaianism—the shift from a normative legal doctrine to a descriptive interpretation of society and culture.

Other endeavors, apart from the tinkering with the Roman system, were operating to transform modern views of jurisprudence. One was the gigantic contemporary enterprise in the field of classical and historical scholarship, although this effort may have served to confuse as well as to broaden perspectives on the old legal tradition. Jacques Cujas was the symbol and leading spirit of this critical assault on the textual aspect of this tradition. Also positive and empirical in a certain sense was the growing appreciation even among academic jurists of the value of modern judicial experience. Cujas’s student, Pierre Ayrault, for example, turned from theory to practice (*usus*) as the best means of access to legal wisdom. For him the true source of law lay in particular judgments (*res judicatae* is the civilian rubric). These Ayrault regarded as the first or only or “supreme” part of the law; and he compiled a modern digest (*pandectae*) of such judgments that followed, of course, the conventional order. “Look to the practitioners,” he advised, “for those things that pertain to their art.”⁵⁵ Finally, there were the long-standing ideological objections to Romanism, especially national jealousy of imperial-papal intrusions and claims to universalism. In France the standard formula was “that the civil laws of the Romans may not be alleged in the courts of France or in any inferior courts on the basis of their authority but only on the basis of their rationality” (*pro ratione*).⁵⁶ In this way, though in few others, nationalism can be said to have reinforced rationalism. In all of these ways the intimidating position of the Roman canon and of Gaianism in a general sense was increasingly undermined, or at least relocated, in a more pluralistic and rational conception of human history and society.

⁵⁴ Grégoire, *Syntagma iuris universi* (Cologne, 1623). Also see his *De iuris arte, methodo, et praeceptis* (Lyon, 1580).

⁵⁵ Ayrault, *Rerum ab omni antiquitate iudiciarum Pandectae*, 1 (Geneva, 1677): 84. On *Digest*, 42. 1 (“*Res judicatae*”), also see Ulrich Zasius, *Opera omnia*, 3 (Lyon, 1550): col. 360. There is no modern study on Ayrault.

⁵⁶ Charles de Grassaille, *Regalium Franciae libri duo* (Paris, 1545), 45.

AT THIS POINT THE GAIAN PALIMPSEST seems to be exhausted: between the historical and empirical assaults on the one hand and the efforts of rationalization on the other, academic jurisprudence could hardly retain its conventional form; it moved, in a sense, from the letter to the spirit of Roman jurisprudence. In general, the seventeenth century was suffused not only with *l'esprit de système* but also with *l'esprit de géométrie*, and these combined with the enthusiasm for the "new science" of Galileo and Descartes to ensure that legal scholarship would be intimidated if not dominated by what has inelegantly been termed "jusnaturalism." For the next two centuries, so it seemed to many observers at that time and since, legal and social as well as political philosophy was captive to this modern idea—"antique-modern," as Otto Gierke preferred to call it—of natural law.⁵⁷ This cast of mind is a deflection from Gaianism. The resurgence and prominence of the idea of "nature" was used in the explanation and legitimation of human categories, social as well as legal (that is, descriptive as well as normative). Indeed, the "state of nature" tended to take over the function both of the moral basis of social behavior and of the historical interpretation of civilization. Thus, the significance of "convention," so essential to Gaius and other Romanists, was subordinated or distorted. Moreover, the principle of authority—and with it the force of custom, prescription, tradition, and even "interpretation" in a usual sense—tended to be overshadowed by the claims of universal reason. The result was that the "law" seemed in many ways to be dehumanized: man himself—his will, his history, his cultural individuality as well as his irrationality and perhaps sinfulness—was increasingly lost to view.

Gottfried Wilhelm Leibniz, who published his own "new method" of jurisprudence in 1667, provides the most extreme example of legal rationalism (*jurisprudentia rationalis*). Although thoroughly grounded in legal scholarship and attached to the "reformed" dialectic associated with Ramus, Leibniz turned to mathematics, specifically to Euclid, for his model of rationality. He rejected the threefold division of Gaius because, as so many others had pointed out, "actions derive from both persons and things." Most fundamental to his objection was the apparently random empiricism of conventional jurisprudence. "Its method," he remarked, "was taken from the inmost parts not of law but of fact" (*haec Methodus non ex Juris sed Facti visceribus sumpta*), "for persons and things are terms of fact, as power, obligation, and the like are terms of law."⁵⁸ The confusion and irrationality inherent in such an approach was too absurd for a modern conception of law: "Who would not laugh at such a new Euclid?" For Leibniz, however, jurisprudence, along with other humanistic studies, was subordinated to metaphysics, ultimately

⁵⁷ Still fundamental, in addition to Stintzing and Landsberg, *Geschichte der deutschen Rechtswissenschaft*, is Otto Gierke, *Das deutsche Genossenschaftsrecht*, the relevant part of which has been translated by Ernest Barker as *Natural Law and the Theory of Society, 1500–1800* (Boston, 1957); but, in general, the subject of natural law is too peripheral and too voluminous to allow bibliographical comment here.

⁵⁸ Leibniz, *Nova methodus discendae docendaeque iurisprudentiae* (Frankfurt, 1667), in *Sämtliche Schriften und Briefe*, 6, pt. 1 (Berlin, 1971): 298. On Leibniz in general, see K. Dickerhof, *Leibniz' Bedeutung für die Gesetzgebung seiner Zeit* (Freiburg, 1941), and F. Sturm, *Das römische Recht in der Sicht von Gottfried Wilhelm Leibniz* (Göttingen, 1968).

to mathematics; and his position represents a pole rather than a school of thought in the range of legal and social doctrines.

The major figure in the re-emergence of natural law was Hugo Grotius, though he, too, was steeped in humanistic and legal erudition. His main legal works were devoted to that anarchic arena of international relations, which itself seemed to represent a pre- or extra-legal "state of nature." Like Leibniz a generation later, Grotius had a basic contempt for unclassifiable particulars. "For the principles of the law of nature," he wrote in the prolegomena to the *Law of War and Peace*, "since they are always the same, can easily be brought into a systematic form; but the elements of positive law, since they often undergo change and are different in different places, are outside the domain of systematic treatment, just as other notions of particular things are."⁵⁹ In a youthful work on prizes and booty he took the same view and, reasoning from a set of general rules and law, affected an even more deliberately mathematical plan.

Yet this renewed emphasis on nature and reason is by no means the whole story of social and legal thought in the age of reason. The *ius naturale* was, after all, an essential part of the Roman legal tradition; and, as Richard Zouche reminded his seventeenth-century contemporaries, Gaius himself had explained that the basis of the law of nations was that "natural reason among men" (*naturalis ratio inter omnes homines*).⁶⁰ Indeed, the continuing association of the *jus gentium* and the *jus naturale* permitted the legal tradition to maintain its human groundings. Grotius, for example, was not only a *jusnaturalist* and "father of international law" but also, as Giambattista Vico later called him, the "jurisconsult of the human race."⁶¹ Like Vico, Grotius neither forgot history nor despised tradition. He persisted in citing ancient "authorities," including not only the Bible, jurists, and scholastic philosophers but also literary and especially historical writers, who provided judgments as well as illustrations. Grotius's intentions were, perhaps, less rationalizing than universalizing; for his primary field of operations was indeed "positive law," specifically the kind of "voluntary human law" (*jus humanum voluntarium*) defined as the law of nations (*jus gentium*, exclusive of the *jus civile* as posited by Gaius). Universal law, according to Grotius, arises not only from nature and from divine ordinance but also from custom or tacit consent—that is, from human will. "And the Law of Nations is proved in the same manner as the unwritten Civil Law," he wrote, "namely, by long usage and the testimony of its professors; for this law, as Dio Chrysostom says, is 'the invention of time and experience,' and here the great historians are of the greatest service to

⁵⁹ Grotius, *De iure belli ac pacis libri tres*, trans. W. Knight (London, 1922), prolegomena, 22, and *De iure praedae commentarius*, trans. G. Williams and Q. Zeydel (Oxford, 1950), 7.

⁶⁰ Zouche, *Iuris et iudicii fecialis* (1650), ed. T. Holland (Washington, 1911), 1. Also see J. W. Textor, *Synopsis iuris gentium*, ed. L. von Bar (Washington, 1916), 2.

⁶¹ See Dario Faucci, "Vico and Grotius: Jurisconsults of Mankind," in G. Tagliacozzo and H. White, eds., *Giambattista Vico: An International Symposium* (Baltimore, 1969), 61–76; L. Rosa, "Grozio fra il giusnaturalismo scolastico e il giusnaturalismo moderno," in *Miscellanea Adriano Gazzana*, 2 (Milan, 1960); Wolf, *Grosse Rechtsdenker der deutschen Geistesgeschichte*; and Fassò, *Storia della filosofia del diritto*.

use.”⁶² In these ways Grotius managed to retain at least indirect contact with the old Roman tradition.

In France at the very height of the enthusiasm for Cartesianism and that “geometric spirit” sensed and to some degree resisted by Blaise Pascal, the persistence of Gaianism is apparent in a sublimated form. The clearest example, as well as the greatest monument of jurisprudence before Robert Pothier, is the work of Pascal’s friend, Jean Domat, whose great system began to appear in 1695. Although Domat’s *Civil Laws . . .* were arranged . . . in *Their Natural Order* (an excellent illustration of the craving to reduce convention or history to nature), they remained, in effect, a rationalizing commentary on Roman experience. And, though he affected to seek out fundamental (natural, perhaps prehistorical) principles, Domat found their human expression in the usual texts of civil law. He gave precedence to the natural over the civil state, but he continued to conceptualize within the Roman paradigm, keeping the rubrics of persons, things, and actions. Convention and not nature, for example, led him to define persons in terms of “liberty” (or the lack thereof), citizenship (or the exclusion therefrom, as in foreign or exiled status), and fatherhood (or subjection thereto).⁶³ The general impression is of rationality, or the rhetoric of rationalism, imposed on the old Roman categories. Even Domat’s ultimate goal, famously defined as the “spirit of the laws,” was a more rationalized version of the old juristic aim of getting at the true meaning.

Montesquieu’s *L’Esprit des lois* of 1748 is, of course, the work that made this phrase and concept universally famous, although the connection with the earlier legal tradition was effectively obscured by the author himself, whose claims to originality have never been disputed and seldom even examined. “My ideas are new,” wrote Montesquieu, “and therefore I have been obliged to find new words, or give new acceptations to old terms, in order to convey my meaning.”⁶⁴ To point out the relation of Montesquieu’s system to the old Roman tradition, which he had surveyed historically in his *Grandeur and Decadence of the Romans* (1734), is not to deny its novelty or conceptual force. His basic concern was not with antecedent “laws of nature” but rather with the old law of nations and civil laws; and, if he construed his subject in an extraordinarily wide-ranging fashion, he followed the lead of Baron, Bodin, and others in adopting a comparative approach, in taking universal history and the law of (all) nations as his field, and in emphasizing climate, geography, and cultural tradition. In more specific ways, too, he retained contact with Roman forms as well as with the texts and modern scholarship of civil law, and the book has with some justice been regarded as a “new *De Legibus*.” As Le Douaren and others who commented consecutively on the *Corpus juris*

⁶² Grotius, *De iure belli ac pacis*, xiv.

⁶³ Domat, *Les Loix civiles dans leur ordre naturel*, 1 (Paris, 1835). On Domat, see R. F. Voeltzel, *Jean Domat (1625–1696)* (Paris, 1936), 107; and, most recently, G. Tarello, ed., *Materiali per una storia della cultura giuridica*, 2 (Florence, 1972): 127–57.

⁶⁴ Montesquieu, *L’Esprit des lois* (Paris, 1748), preface. On the problem of the work’s structure, see Franz Neumann’s introduction to the English translation, *The Spirit of the Laws*, trans. T. Nugent (New York, 1949), and, more recently, Mark Waddicor, *Montesquieu and the Philosophy of Natural Law* (The Hague, 1970); S. Goyard-Fabre, *Philosophie du droit de Montesquieu* (Paris, 1973); and Tarello, *Materiali per una storia*, vol. 1.

began with a definition of the law, its species, and its sources (*quid jus, de divisione juris, and unde jus*), so Montesquieu began with general definitions, continued with the species of constitutions (corresponding to democratic, aristocratic, and monarchical sources of law) and then likewise proceeded to the law of persons (liberty and servitude) and of things (commerce), family, and succession. Montesquieu also ended up with a discussion of feudal law (corresponding to the *Libri feudorum*, which modern jurists had accepted as a continuation of “Roman law”). Although Montesquieu’s book may have been “without a mother” (*prolem sine matrem creatam* was its motto), it did have a kind of conceptual godfather: the “spiritual consanguinity” of the basic Roman paradigm. *The Spirit of the Laws* can be read as a set of variations, however remote and figurative, on Gaian and Justinianian themes.

As Cartesianism did not entirely efface the old Roman canon in France, *Naturrecht* did not exclude the study of positive law or even end the tradition of Gaianism in Germany. Most representative of jusnaturalism in Germany was the encyclopedic work of Johann Gottlieb Heineccius, though he belonged as well to the continuing enterprise of humanistic scholarship and looked back in particular to Le Douaren. Like Le Douaren, Heineccius worked along both philological and philosophical lines. His historical surveys of civil and Germanic law and his various investigations into crucial topics of modern critical jurisprudence—including Justinian’s prohibition against “interpretation,” the ignorance of Greek (that is, the “specious dictum,” *Graecum est, non potest legi*), the “anti-Tribonianist sect,” and biographical sketches of Cujas and other jurists—exemplify his philological analysis.⁶⁵ The second line of endeavor consists of a series of commentaries—“Elements,” he called them in Euclidean style—not only of philosophy in general but also of natural law, the law of nations, and especially civil law, from the Twelve Tables down to his own day. The Gaian tradition is represented by his *Syntagma* of 1718, a systematic treatise “according to the order of the *Institutes*,” but also making use of Oldendorp and other modern scholars.⁶⁶ In general, his first concern was with the human condition (*de statu hominis*), and he expressly repudiated Thomas Hobbes’s naturalistic tendency to derive all law from contractual agreement. For Heineccius the mainstream of wisdom, though it certainly originated in Greek philosophy, was that “knowledge of things divine and human” (the title of another of his dissertations) identified with jurisprudence.⁶⁷ In a number of ways Heineccius—like Grotius, Domat, and others—provides a link between enthusiasm for natural law and older traditions of positive scholarship—and also, not accidentally, between the old jurisprudence and newer views of how to go about studying society.

The importance of the Roman experience in the thought of the Enlightenment is most conspicuous in the historical works of Montesquieu and Edward

⁶⁵ Heineccius, *Opera omnia*, 4 vols. (Geneva, 1744), 3: 17, 21, 171, 203. On Heineccius, see Stintzing and Landsberg, *Geschichte der deutschen Rechtswissenschaft*, 3: 179–97.

⁶⁶ Heineccius, *Antiquitatem Romanarum iurisprudentiam illustrantium syntagma secundum ordinem Institutionum Justiniani digestum*, in *Opera omnia*, 4. Also see *Opera omnia*, 1: 191.

⁶⁷ Heineccius, “De iurisprudentia divinarum humanarumque rerum notitia,” in *Opera omnia*, 3: 374–89.

Gibbon, but it was not only decadence and decline that fascinated scholars. The positive force of Gaianism persisted in a variety of ways and so did the grand tradition of Renaissance scholarship that had accumulated around Roman law. François de Boutaric, like Loisel more than a century earlier, offered a comparative analysis of French customary law within the framework of Justinian's *Institutes*.⁶⁸ And Damiano Romano, in the fashion of Bodin almost two centuries earlier, offered a treatise "on the true law of nature and of nations" that was organized according to universal history and drew upon the scholarly work of François Hotman, Cujas, Doneau, Le Douaren, and many other, especially Protestant, jurists of the sixteenth and seventeenth centuries.⁶⁹ Other examples of adherence to older scholarly traditions can be found among jurists who ostensibly belonged to the natural school but who were repelled by the naturalistic excesses of Hobbes, Samuel von Pufendorf, Christian Thomasius, and Wolf. This repugnance was especially clear in the continuing efforts to improve international law, the roots of which went back directly to Gaius via Grotius. Gaian notions also continued to inform encyclopedic enterprises; for instance, Joachim Daries' *Institutions of Universal Jurisprudence*, somewhat like Grégoire de Toulouse's *Republic*, proposed to describe the nature of man, society, and public and private law in a "systematic," "methodical," and "scientific" fashion, yet again following the old Roman forms.⁷⁰

But the clearest expression of Gaian influence during the age of Enlightenment was surely the effort to codify positive law, which came to maturity during the next century and, indeed, became a central political issue in Savigny's time. Once again the center of attention was France, where the codification movement had arisen in the sixteenth century in connection with the work of Charles Dumoulin and other "systematic" jurists already discussed. What was required for such an enterprise to succeed, according to A. -J. Arnaud, a recent historian of the movement, was the "juridical rationalism" and "modern philosophy" of the age of Descartes, especially the work of Domat and his eighteenth-century successors, including Montesquieu but even more Pothier and Henri Daguesseau, who were more directly significant for the Napoleonic Code.⁷¹ Yet, as Arnaud himself has shown, the "order" achieved by modern philosophy retained much of its Roman composition, especially the tripartite Gaian scheme, which carried over into the French civil code. The "philosophic" ideal of the rationalizing jurists, of course, was itself a central feature of the Roman legacy. Antoine Terrasson was, perhaps, "the first historian of law really concerned with the philosophy of law"; but

⁶⁸ De Boutaric, *Les Institutes de Justinien conférés avec le droit français* (Toulouse, 1738). In general, see Klaus Luig, "Institutionen-Lehrbücher des nationalen Rechts im 17. und 18. Jahrhunderts," *Ius Commune*, 3 (1970): 64-97.

⁶⁹ Romano, *Del vero diritto della natura e delle genti* (Naples, 1757).

⁷⁰ Daries, *Institutiones iurisprudentiae universalis in quibus omnia iuris naturae socialis et gentium capta in usum auditorii sui methodica scientifica explanantur* (3d ed., Jena, 1748).

⁷¹ Arnaud, *Les origines doctrinales du Code civil français*, 27, *passim*; and Ph. Sagnac, *La Législation civile de la révolution française* (Paris, 1898), 51.

Terrasson's history is specifically of Roman law as an embodiment of this philosophy. And, when he wrote, "Philosophy is the true source of jurisprudence," he was thinking of Ulpian, not of Descartes or Leibniz.⁷² In any case, what was really required to achieve a code in France was not a conceptual effort; it was a revolution.

PARALLELING THESE RATIONALIZING TENDENCIES in the eighteenth century were various and increasingly obvious survivals of older humanistic traditions. Indeed, this resurgence of historical and literary erudition divided the Enlightenment from the earlier "age of reason." Despite attacks by naturalizing philosophers on "authority" (and, by inference, on history), positive legal and literary scholarship continued to flourish among a variety of unfashionable *érudits*, who refused to allow the new philosophy to cast everything in doubt. Neither Descartes's skepticism nor Galileo's scorn for historians ("memory experts") could stem the flow of antiquarian social, legal, and institutional history. In Germany Hermann Conring, Heineccius, and Leibniz (!), in the Netherlands Perizonius and Grotius, in France Jacques Godefroy, Terrasson, and Montesquieu, in Italy Giovanni Gravina and Ludovico Muratori, and even in England John Selden and Henry Spelman carried on the great enterprise begun by earlier humanists.⁷³ Without them, it is too seldom recalled, the work not only of Gibbon and other historians of the Enlightenment but also of Niebuhr and Savigny, however original and ground-breaking, would have been impossible. Indeed, Savigny's famous manifesto of the Historical School, "The Vocation of Our Age for Legislation and Jurisprudence" (1814), may be regarded as a celebration of the monumental efforts of this (philosophically) tenuous tradition of scholarship as well as an assault on naturalism and its attendant fallacies.⁷⁴ Under cover of these tendencies, Gaianism could maintain a certain precarious existence even outside of narrow professional circles.

In fact, resistance to a physical, or metaphysical, approach to the study of humanity long antedated the rise of the nineteenth-century historical schools. The first great nemesis of radical naturalism was Giambattista Vico, who was at the same time a leading champion of Roman jurisprudence. Vico waged his campaign not only against the current Cartesian form of naturalism but also against its earlier philosophical manifestations. For him a true "science" of humanity had to accommodate not only nature but also convention, not only reason but also authority (and unreason), not only determinism (*necessitas naturae*) but also the free and creative wills of men. As Le Douaren, Lagus,

⁷² Terrasson, *Histoire de la jurisprudence romaine* (Paris, 1750), 101.

⁷³ In addition to Stintzing and Landsberg, *Geschichte der deutschen Rechtswissenschaft*, volume three, there are various specialized studies. For two notable ones, see C. Chisalberti, *Gian Vincenzo Gravina, giurista e storico* (Milan, 1962); and K. Kossert, *Hermann Conrings rechtsgeschichtliches Verdienst* (Cologne, 1939).

⁷⁴ See, for example, Savigny, *Das Recht des Besitzes*. For other works by and about Savigny, see notes 80–81, below.

Grotius, and many others had said, human science had to combine historical-philological with philosophical investigations. And according to Vico the model for such a science was neither Greek philosophy nor rhetoric, which had unfortunately remained conceptually separate, but instead that characteristic Roman kind of "wisdom" (*sapientia*) that was jurisprudence—or, rather, the modern expression of this "civil doctrine" born of humanism and best expressed by Grotius.⁷⁵ In the context of Vico's epistemology, itself derived from or, at least, analogous to civil law, the "new jurisprudence" combined the study of the cultural products of human will, the "certain," with the study of the products of God's will, or nature, which was the "true." It had to combine, in other words, an understanding of "authority," or history, with a search for "reason," or the divine ideal, in just the same way that philosophical jurists proceeded. Such was the argument of Vico's treatise, *Universal Law*, which was indeed the first version of the *New Science*—the bottom level of an intellectual palimpsest hardly less complex than Gaius' own.

Vico was one of the great transformers as well as champions of jurisprudence. His method—indeed, his obsession—to reformulate everything into historical terms meant he could not directly take over the static Gaian triad. Yet Vico himself was notoriously a trichotomizer; and his own neotrinitarian structures displayed certain analogies with those of Gaius. On the level of epistemology, or psychology, the Vichian classes were *nosse*, *velle*, and *posse*; and they might be rendered respectively as self-consciousness (perhaps self-possession or self-control), vitality (or the urge to live and to relate to others), and activity (or desire to act and to achieve). In the course of the historical process these three basic faculties of human nature were transformed into what can be considered as social, beyond a mere individual, consciousness (*mens*), rationality (*ratio*), and awareness of divinity (*Deus*). In the sphere of civil society, another homologous triad, the institutional categories, again were produced by historical development: *dominium*, which can be understood as control of things, the basis of civil possession, or property; *libertas*, in social terms the legal order, the rule of law; and *tutela*, active control over and direction of society on behalf of its values and goals—that is, government.⁷⁶

It will not do to make too close an identification between the Vichian and the Gaian systems. For one thing Vico was inordinately proud of the "geometric" design of his "new science," and increasingly the Romanoid structure was lost in the successive and increasingly sublimated versions of the system. But both the *Universal Law* and the *New Science* contained a common denominator: both systems retained, on the one hand, a primary category of personality (that is, the subject of rights and obligations) and, on the other, a secondary category of natural reality (that is, objective "otherness," which represented the field for acquisition—both natural and civil possession). In

⁷⁵ For a fuller treatment of the place of rhetoric, see my "Vico's Road: From Philology to Jurisprudence and Back," in G. Tagliacozzo and D. Verene, eds., *Giambattista Vico's Science of Humanity* (Baltimore, 1976), 15–29.

⁷⁶ Vico, *De universi juris uno principio et fine uno*, ed. F. Nicolini (Bari, 1936).

both systems actions were defined in terms of these two classes. These remarks hardly begin to reveal Vico's relations to the legal tradition, not only to the ancient sources but to the scholarship of Domat, Grotius, the French "systematists," and other commentators of the preceding two centuries; they may, however, serve to suggest some of his importance as a link between jurisprudence and a modern philosophy of society and culture—another way in which a traditional legal science could become the point of departure for a modern social science.

In various ways Vico anticipated, even if he had no direct impact on, the new view of jurisprudence that began to appear in the late eighteenth century—the Historical School of Law, which became more fully defined, and in some ways even dominant, in the wake of the Napoleonic wars. The point of departure of this school was a historically minded—whether conservative, nationalistic, or "romantic"—rejection of theories of natural law or, at least, a reformulation of them. Its home territory was, of course, Germany, where the wars of liberation were directed not only against Napoleonic domination but also against Bonapartist and Jacobin ideology.⁷⁷ To simplify a very complex matter, the assault on rationalism and jusnaturalism (and, related to this attack, the emergence of the Historical School) took its strength from three interrelated traditions. First, the continuing efforts of antiquarian research into the national as well as the classical past followed the work of Heineccius and earlier historians and ultimately stemmed from the philological achievements of Renaissance humanism. Second, again related to sixteenth-century concerns with practical matters, "cameralistic science" (whatever its politics and goals) acted as a solvent upon grand theories of natural law, whether older rationalist schemes or newer idealistic constructs, such as that of Kant. Third, most immediate and most emotional was opposition to that concrete embodiment (or, at least, symbol) of oppressive and arbitrary systems, the Napoleonic Code (and, to some extent, its earlier Prussian and Austrian counterparts). These were the grounds on which the *Historische Rechtsschule* was established in the early nineteenth century.

The man most often regarded as the founder of this school—*Altervater*, Marx called him, and *Naturmensch*—was Gustav Hugo, whose attitudes stemmed quite directly from the tradition of classical humanism. Among other works, he published a new edition of the famous fragments of Ulpian (first published by Cujas and referred to by both Montesquieu and Vico) and a translation of Gibbon's forty-fourth chapter surveying the history of Roman law, likewise based on humanist scholarship; he also wrote voluminously on and taught legal history. He began publishing his systematic and historical works in 1789, taking as his special target "dogmatic" jusnaturalism and setting in its place his own realistic sort of natural law.⁷⁸ His system was founded on a "juristic

⁷⁷ Like Gaius himself, the *Historische Rechtsschule* has spawned a literature too voluminous to mention and a long concatenation of journals starting with those of Savigny and Hugo and ending with the *Zeitschrift der Savigny-Stiftung für Rechtsgeschichte*. See, for example, Georges Gurvitch, *L'Idée du droit social* (Paris, 1932).

⁷⁸ Hugo, *Lehrbuch des Naturrechts als eine Philosophie des positiven Rechts*, which was part of his *Lehrbuch eines civilistischen Cursus*, which began to appear in 1789.

anthropology," in which he considered the animal characteristics of persons (*der Mensch als Tier*) and went on to consider law as "natural" in the sense not of rationality but of a *de facto* growth. In one sense, Hugo recapitulated the method of Gaius, who likewise accepted without question the "reality" of past Roman experience, thereby giving "authority" to history, and then rationalized it through dialectical argument. Not surprisingly, Hugo was, like Gaius, open to charges of being arbitrary and authoritarian; and, as conventional Romanism came under fire from rationalists like Leibniz, so the Historical School was attacked by idealists—especially Hegelians, the most famous of whom was the young Marx. Although he recognized that Hugo represented a "reaction against the frivolous spirit of the eighteenth century," Marx objected to the irrationality of the alternative. About Hugo's historical method Marx observed, "Everything serves him as an authority, every authority serves him as an argument"; and about the true principles of justice he concluded that Hugo "desecrates them in the eyes of reason in order afterwards to make them honorable in the eyes of history."⁷⁹ So Hugo seemed even to justify slavery; and, in general, Marx reduced Hugo's philosophy of law to the proposition that "the sole juridical feature of man is his animal nature."

Savigny, Marx's teacher, who first joined and then superseded Hugo in the leadership of the Historical School, regarded this attack, of which he was only an indirect target, as unfair and obtuse. Throughout his life Savigny rejected the charge that his view supported the "tyranny of the past" (*Herrschaft der Vergangenheit*), and he argued that the basis of his thought was not authority but realistic human perspective. His views were expressed first in his youthful yet magisterial treatise on possession, *Das Recht des Besitzes* (1803).⁸⁰ Here Savigny decisively contributed to a discussion that had been going on for centuries, a discussion that had indeed begun with Gaius and that included other jurists, especially Le Douaren, Doneau, and Pothier. The controlling assumption of this book was that practical questions of law had to be settled in terms of Roman law understood not only systematically but also historically—that is, in terms of a process that included not only ancient forms but also medieval interpretations and modern (*heutige*) transformations. Savigny later followed this method in his treatise on the law of obligations and, above all, in his *System of Modern Roman Law*; presumably, he also maintained the same stance in his teaching and in his career as minister of justice for the reform of Prussian law. Savigny was involved, of course, in other contemporary controversies, most notably that between the "idealists" (especially the Hegelians) and the "realists," with whom he was identified; but the extent to which his ideas can be understood in terms of the longer legal tradition as well as the more immediate context of German philosophy is surprising.

⁷⁹ Marx, "Das philosophische Manifesto der historischen Rechtsschule," in *Marx-Engels Gesamtausgabe*, 1, pt. 1 (Berlin, 1927): 251–59. Also see H. Jaeger, "Marx et Savigny," *Archives de Philosophie du droit*, 12 (1967): 65–89; and J. Blühdorn, "Naturrechtskritik und 'Philosophie des positiven Rechts,'" *Tijdschrift voor Rechtsgeschiedenis*, 45 (1974): 3–17.

⁸⁰ Savigny, *Das Recht des Besitzes*, 124, and A. Rudorff's addition, 543–62. Also see Vogt, *Franciscus Duarenus*, 89–108.

From the first, Savigny, like Gaius, had systematic ambitions; and his age, like that of Gaius, was similarly divided, both ideologically and pedagogically, over jurisprudence. The basic question of the suitability of a modern code of laws for German society was the issue on which Savigny and the Historical School rose to prominence.⁸¹ Savigny ridiculed the “positivist” notion advanced by A. F. J. Thibaut of constructing an abstract and academic code as simplistic and outmoded naturalism already discredited historically by the evident failures of the Napoleonic, Prussian, and Austrian codes of the previous half-century. Arguing that law, like language, was the product of a long and gradual development, Savigny concluded that the answer lay in the tradition of Roman law, which had been officially “common law” for three centuries and, in fact, for longer than that. As a result, he came to replace Hugo as the leader of the Historical School, especially after the publication of his manifesto of 1814 and the founding of his journal, *Zeitschrift für geschichtliche Rechtswissenschaft*, in the following year. The premise of this journal, that a true “science” must be the product of centuries of development, was another illustration of the affinity between Savigny and Gaius. Thus, it is appropriate that in the next year came the miraculous discovery of that most ancient relic and deepest root of Roman legal science—Gaius’ *Institutes*.

IRONICALLY, GAIUS APPEARED TOO LATE to be of practical value for the science of jurisprudence; it was too late in the evening for the Roman owl to take flight. The principal significance of the discovery lay in the field of historical scholarship and Roman antiquities. Even more ironically—tragically, in fact, for Savigny—the Historical School was itself more important in the long run for academic—scholarly and philosophical—than for professional achievements. In a sense this culmination of a long process of “historicizing” (*Historisierung*, as a recent historian of eighteenth-century universities calls it⁸²) underlies the great works of Theodor Mommsen, Rudolf von Jhering, and others of the next century. In fact, Savigny’s reputation today rests largely on his massive *History of Roman Law in the Middle Ages*, even though it was intended originally to be a preliminary study of the sources of law and, thus, ancillary to its author’s professional aims and ambitions. Nevertheless, Savigny’s *System of Modern Roman Law* was the principal monument of the Historical School; and in many ways it proposed, in modern terms, to carry out the same sort of task, philosophical as well as pedagogical, that Gaius had set for himself. In a sense, it was a move back toward Gaianism.

Savigny’s neglected masterwork represents not only the culmination of the Historical School but also a kind of historically oriented *summa* of the legal

⁸¹ Savigny, *Vom Beruf unserer Zeit für Gesetzgebung und Rechtswissenschaft* (Heidelberg, 1814), published with Thibaut’s article and other material in J. Stern, *Thibaut und Savigny* (Darmstadt, 1959). On the problem of codification, see J. Vanderlinden, *La Concept du code en Europe occidentale du XXIII^e au XIX^e siècle* (Brussels, 1967); G. Tarello, *Le Ideologie della codificazione nel secolo XVIII* (Genoa, 1971); W. Ebel, *Geschichte der Gesetzgebung in Deutschland* (2d ed., Göttingen, 1958); S. Gagnér, *Studien zur Ideengeschichte der Gesetzgebung* (Stockholm, 1960); and Koschaker, *Europa und das römische Recht*.

⁸² Notker Hammerstein, *Jus und Historie* (Göttingen, 1972), 216.

tradition going back to the classical period of jurisprudence—most specifically to Gaius himself, who figures prominently in as well as provides the prototype for this treatise. Like Gaius, Savigny proposed to give shape to a vastly “rich heritage” through critical and selective judgment.⁸³ He rejected the current fallacy of both historical and legal scholarship, which assumed that a systematic treatment could merely be a collection of monographs (though that treatment, indeed, drew upon such works, including Savigny’s own on possession and obligation). “Scientific” jurisprudence was the creation not of amateur philosophers like Cicero (or even Montesquieu, despite his training) but only of men of professional authority (*auctoritas prudentium*) like Gaius and Savigny himself. To Savigny’s system in general Gaius had made at least three contributions. One was the transmission of particular formulas derived from more ancient Roman legal wisdom. Another was the distinction between the two basic kinds of law—that is, civil law and the law of nations, which was properly identified with natural law (as Savigny thought, in contradiction to those who recognized it as a third type) since it was based on “natural reason.” And, almost unavoidably, the third was the notorious Gaian triad. Although Savigny had critical remarks to make about the triad, he did incorporate it into his own structure. What is more, he went on to point out a number of analogous trichotomies of Roman law that history, if not nature, had endorsed.

At the center of Savigny’s (as of Gaius’) cosmos was, of course, the individual “person,” the first member of the Gaian trinity, which perhaps not originally but eventually opened up the whole question of the “status” and “condition” of man in a philosophical as well as a legal sense. In this connection he referred to Doneau, one of the most important of his systematizing predecessors, who elaborated on the concept of personhood by distinguishing aspects not only of life and security but also of liberty and belief (*existimatio*), all of which stemmed from an “original” rather than an “acquired” right. As Savigny described the basic *conditio hominum*, “Man stands in the midst of the outer world, and the most important element, to him in this surrounding of his, is the contact with those who are like him, by their nature and destination.” From the individual Savigny moved to the social sphere, where as he put it, “in the richness of living reality all jural relations form a systematic whole.” On the level of this web of jural relations (*Rechtsverhältnisse*), the counterpart of the individual was the “people” (*Volk*—that is, the Roman *gens*), and the law thereof was an expression of the old law of nations (*jus gentium*, rendered as *Volksrecht*).⁸⁴ Then, in Gaian fashion, he took up the law of things, actions, and their various derivatives and interactions. Romantic and organicist imagery aside, Savigny’s strategy was quite in keeping with the Romanist tradition, which he was, in his “historicist” way, espousing.

⁸³ Savigny, *System des heutigen römischen Rechts*, ix, 413, 393, 38. And, for a very important article, see Aldo Mazzacane, “Savigny et la storiografia giuridica tra storia e sistema,” in *Scritti in onore di Salvatore Papi*, 4 (Milan, 1978): 515–29.

⁸⁴ Savigny, *System des heutigen römischen Rechts*, 357, 331, xix, 17.

Savigny's influence, though restricted in the long run to the intellectual sphere (his struggle against modern jusnaturalism was defeated with the adoption of the German civil code in 1900), was profound and in many ways pervasive, illustrating again the humanist commonplace that Rome, although it had collapsed as a political structure, continued its dominance through linguistic, literary, and especially legal channels. Gibbon opened his forty-fourth chapter with the observation that "the public reason of the Romans has been silently or studiously transfused into the domestic institutions of Europe . . .," but even deeper and more enduring has been the effect upon legal, historical, and social thought. Before Savigny this Roman influence was most conspicuously evident in the work of Montesquieu and Vico, who had also striven to recapture the essence of the tradition and give it modern form; after him, it was evident in the work of many others, not only disciples but also critics like Rudolf von Jhering, whose magisterial *Spirit of Roman Law* was devoted to the same purpose. To illustrate this purpose, both Vico and Jhering quoted the famous lines of Vergil,

Remember, O Romans, you keep universal rule over nations
In these ways: by maintaining peace through law,
By doing justice to the lowly, by bringing down the haughty. . . .⁸⁵

This prophecy might be taken as the epitaph of Savigny and his school—not only in their efforts to reform and to regulate society but also in their impact on modern social thought in general.

THE ACHIEVEMENT OF THE HISTORICAL SCHOOL had repercussions far beyond the legal scholarship of Savigny, Johann Eichhorn, and their disciples and the parallel historical writings of Barthold Georg Niebuhr and of Leopold von Ranke and his academic offspring. Based upon a total and "organic" view of culture, this approach involved many other areas of cultural study, most notably language, religion, and political economy. Out of the interdisciplinary matrix created by these complementary and overlapping "historical schools" emerged one phase of a new discipline called sociology. One of the first historians (and in this country a founder) of that discipline pointed to the Historical School of Law, especially to the Savigny-Thibaut controversy, as seminal for the growth of sociology.⁸⁶ A more concrete illustration of this connection is Marx, whose social thought arose in the context of his legal education and, specifically, in the quarrel between the Hegelians and the Historical School; and a number of his ideas, including class structure and "alienation," were directly linked with the civilian tradition.⁸⁷ Other particular instances could be added—including, perhaps, the systematic work of Max Weber and, certainly, his sociology of law.

⁸⁵ Vico, *Diritto universale*, 214; and Rudolf von Jhering, *Der Geist des römischen Rechts auf den Stufen seiner Entwicklung*, 1 (Berlin, 1852): 306.

⁸⁶ Albion Small, *Origins of Sociology* (Chicago, 1924), 37–62.

⁸⁷ See my article, "The Metaphysics of Law: An Essay on the Very Young Marx," *AHR*, 83 (1978): 350–67, which is a continuation of the present discussion.

A central remaining question is the significance of the old Roman and Gaian paradigm for modern social thought, especially in its systematic sociological forms. Although to provide an answer moves the discussion from the historical dimension to transhistorical speculation, the question at least provides a logical *terminus ad quem* for this discussion of the afterlife of Roman forms and of Gaianism in particular. To conclude with a retrospective glance at this tradition, it seems clear that the first, and perhaps the most fundamental, continuing influence was pedagogical. Civil law was a major vehicle of secular higher education in European universities from the thirteenth to the nineteenth century (as it had been in Gaius' time), and the *Institutes* (first in a Gaian, then in a Justinianian, and finally in a national form for various European states) constituted one of the basic textbooks of Western civilization, arguably the single most influential introduction to social thought. Certainly, the overwhelming majority of social thinkers prior to Savigny were juridically trained. Secondly, Roman civil law was an almost inexhaustible source of assumptions, insights, terminology, methods, formulas, and concepts for systematic thinking about social structure and process; and even for those who, like Marx, reacted against it, the importance of civil law in negative ways was considerable. Finally, through its ideals, systematic form, and practical intentions, Roman jurisprudence furnished a "scientific" and "philosophical" model for the understanding of human society. The specific relationships between the old legal tradition and the new fields of human study, including economics, sociology, and anthropology, for the most part still await investigation.

In assessing the significance of civil law in Western social thought, let us, in conclusion, return to the original epistemological basis of the Gaian tradition. The ancient dualism of nature and convention implied a distinction between the natural and the social or historical condition of man; and Gaius' subjective and "personal" starting point gave priority to the latter, in contrast both to the intimidating political philosophy of Aristotelianism and to modern naturalism. Jurisprudence could not extricate itself from the conventional, however much it reached toward the natural: as "true philosophy," it was incorrigibly man-centered, value-laden, and action-oriented. It had to view the human condition not as a natural process or a logical construct but as a human epic or drama that focused on the conflict of wills and the means of resolving them within the overall communal pattern. Ultimately, perhaps, the natural and the conventional—reason and custom, the "spirit of law," and the reality of society—might be reconcilable; but men cannot judge, act, and live "ultimately," though they can talk about doing so. Of this human yet global approach to "science," the symbol and in some contingent (and "conventional") sense the starting point is "our Gaius."

Frontier Municipal Baths and Social Interaction in Thirteenth-Century Spain

JAMES F. POWERS

THE TWELFTH AND THIRTEENTH CENTURIES were pivotal in the history of the Iberian Peninsula. The Reconquest created several large Christian principalities in the territory recovered from Islam, imposing a large number of northern Spaniards upon the south and fundamentally shaping the outlook of peninsular natives from that day to this.¹ As the Christian states drove to the south in an uneven series of thrusts, the Reconquest drained off the northern population, a drain which was never entirely replaced by extrapeninsular colonists and crusaders. During the struggle for the Guadiana and lower Ebro valleys in the mid-twelfth century, the north's population shortage made it difficult to find sufficient colonists for the conquered regions. The rulers of León, Castile, Aragón, and Catalonia were compelled to attract migrants by granting liberal municipal charters—*fueros*—that enlarged personal freedoms and immunities. The conquered towns of the south drew their Christian populations from an adventurous, mobile segment of the northern population willing to relocate on the edge of European civilization. In the southern municipalities aggressive Christian conquerors came into daily contact with an older, more urbane, and recently defeated native populace.

The farther south the northern Christians penetrated, the older and deeper were the Arab and Berber civilizations they encountered. The Iberian municipalities that were the scene of this Muslim-Christian confrontation have received some attention from scholars over the last several decades, although a fresh examination in depth is still badly needed.² The exploration of munici-

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¹ An excellent brief review of this movement now exists in English; see Charles Julian Bishko, "The Spanish and Portuguese Reconquest, 1095-1492," in Harry W. Hazard, ed., *The Fourteenth and Fifteenth Centuries*, vol. 3 of Kenneth M. Setton, ed., *A History of the Crusades* (Madison, Wisc., 1975), 396-436. For contrasting interpretations of the Reconquest, see Joseph F. O'Callaghan, *A History of Medieval Spain* (Ithaca, N.Y., 1975), 193-427; and J. N. Hillgarth, *The Spanish Kingdoms, 1250-1516*, vol. 1: *Precarious Balance, 1250-1410* [hereafter *The Spanish Kingdoms, 1250-1410*] (Oxford, 1976), 1-15, 21-29.

² Antonio Sacristán y Martínez's *Municipalidades de Castilla y León* (Madrid, 1877) has been the standard account, but it is badly dated. For a recent study of the municipal town council, see María del Carmen Carlé, *Del concejo medieval castellano-leones* (Buenos Aires, 1968). There have been numerous city histories,

pal social history has only begun. In these towns one institution, the public bath-house, stands out as a vital social enterprise. No public facility was more personal, required a more complex pattern of usage, and established the need so intensely for an effective body of governing regulations. The Roman or Mediterranean heritage of public bathing was common to both Christian and Muslim Spaniards, although trans-Pyrenean bath-houses were not unknown in the Middle Ages, but much of the European evidence dates from later than the twelfth century. Roman influences, moreover, were more diffuse in the north, where the Christian Iberian principalities were born. The public bath-house flourished more in Muslim Spain. At least the remaining evidence for the period leading up to the twelfth century renders far more in the way of Muslim than Christian precedents.³

The standard secondary account of Christian usage of public bathing facilities is a brief article by Aníbal Ruiz-Moreno published in 1945.⁴ Ruiz-Moreno described the baths as facilities that brought together the religious minorities in a shared public activity, and he emphasized the pre-twelfth-century background of Christian bathing. The origins of these municipal bath-houses has also contributed to a strong disagreement between two eminent historians of Spain, Américo Castro and Claudio Sánchez-Albornoz,⁵

but they have been rather uneven in quality. A number of historians have attempted various assessments of the impact of required military service upon the lives and urban organization of townsmen. See Antonio Palomeque Torres, "Contribución al estudio del ejército en los estados de la Reconquista," *Anuario de Historia del Derecho Español* [hereafter *AHDE*], 15 (1944): 205-351; J. M. Lacarra, "Les villes-frontières dans l'Espagne des XI^e au XIII^e siècles," *Moyen Âge*, 69 (1963): 205-22; and James F. Powers, "Townsmen and Soldiers: The Interaction of Urban and Military Organization in the Militias of Mediaeval Castile," *Speculum*, 46 (1971): 641-55, and "The Origins and Development of Municipal Military Service in the Leonese and Castilian Reconquest, 800-1250," *Traditio*, 26 (1970): 91-111.

³ Robert I. Burns, "Baths and Caravanserais in Crusader Valencia," *Speculum*, 46 (1971): 453-58. Burns has provided an excellent brief commentary on Mudejar bathing and useful bibliographical notes. Also see his *Medieval Colonialism: Postcrusade Exploitation of Islamic Valencia* (Princeton, 1975), 57-63. For a comparison in time and place in the Muslim world, see André Raymond, "Les bains publics au Caire à la fin du XVIII^e siècle," *Annales Islamologiques*, 8 (1969): 129-50.

⁴ Aníbal Ruiz-Moreno, "Los baños públicos en los fueros municipales españoles," *Cuadernos de Historia de España* [hereafter *CHE*], 3 (1945): 152-57. For Leopoldo Torres Balbás's commentary on this article, see "Los baños públicos en los fueros municipales españoles," *Al-Andalus*, 11 (1946): 443-45. Although helpful as far as it goes, Ruiz-Moreno's study is limited by a point of view that seeks only to establish that Spaniards bathed in the Middle Ages (a long-delayed refutation of Richard Ford and Karl Baedeker), by its incomplete survey of the available resources, and by its failure to ask modern social questions of the intriguing charter materials. Ruiz-Moreno's work has long needed revision and supplementation. For the rest of Spain, there are some brief summaries that concentrate upon Catalonia: J. Ernest Martínez-Ferrando, *Baixa edat mitjana (siegles XII, XIII, XIV, i XV)*, vol. 3 of Ferran Soldevila, ed., *Història dels Catalans* (Barcelona, 1961), 1609-13; and F. Carreras y Candi, *Miscelanea històrica catalana*, 1 (Barcelona, 1905): 200-04. For the most recent summary for all of the Spanish kingdoms, see Hillgarth, *The Spanish Kingdoms, 1250-1410*, 189-90.

⁵ For the evolution of this debate, see Castro, *España en su historia: Cristianos, moros, y judíos* (Buenos Aires, 1948), 83-84, *La realidad histórica de España* (Mexico City, 1954), 116-18, *The Structure of Spanish History*, trans. Edmund L. King (Princeton, 1954), 107-09, and *The Spaniards: An Introduction to Their History*, trans. Willard F. King and Selma Margaretten (Berkeley and Los Angeles, 1971), 272-73; and Sánchez-Albornoz, *Estampas de la vida en León durante el siglo X* (Madrid, 1926), 122-23, and *España, un enigma histórico*, 1 (Buenos Aires, 1957): 194-95. Castro devoted much of his life to developing the thesis that Spain was born in the central Middle Ages growing out of tripartite Christian, Muslim, and Jewish interlocking social structures and that Spanish society was severely disrupted when the Muslims and Jews were persecuted and expelled during the early modern era. Part of Castro's argument rests on the influence of the baths as an Arabic institution accepted by the Christians who encountered it during the Reconquest. In 1957 Sánchez-Albornoz countered with additional evidence of Christian bathing unrelated to Muslim influences both in

who have debated whether municipal public bathing was Christian or Muslim in origin. But they did not closely examine the actual Christian, Muslim, and Jewish patterns of usage of the *baños* in the thirteenth century.

Another important, although secondary, theme that has emerged in the study of the municipal bath is a concern for the honor and the security of women utilizing the facility. The need for female settlers during the twelfth-century expansion was particularly acute. Continuing warfare on the frontier reduced the number of available men; as a consequence, women were permitted to inherit property and titles and thereby secured new generations on the frontier—a situation comparable to that in the Near East during the Crusades. But these privileges did not match those granted to men, and the insecurity of life in frontier towns tended to keep women in short supply.⁶ Although many of the urban *baños* were privately owned, they were open to the public: men and women, Christian and non-Christian. Therefore, women were dealt with in the laws of the bath-house as though they, too, were a special minority. In their case the law was especially protective, almost excessively so.

The *baños* provided a challenge to the lawmakers by presenting social situations that were unique and potentially threatening. Through the eyes of the redactors of the charters, vital insights can be gained into the milieu of these newly conquered towns and the social restraints under which the Christian society operated. Because the clerics who wrote most of the contemporary chronicles had little interest in townsmen, their accounts provide little information about municipal life and its problems. The surviving charters, however, provide a resource that has not been fully mined. To be sure, these charters (usually identified by the Latin *forum*, the Castilian *fuero*, and the Catalan *fur*) are limited: they tend to be brief listings of rights and privileges wrung from kings, nobles, and churchmen early in the Reconquest; not until the later twelfth century did these documents begin to evolve into fuller statements of municipal law that contain material useful to social historians. But Spanish scholars have been inclined to focus upon the evolution of regional law, a subject that is in itself limited and tends to fragment an overall understanding of municipal trends. Some regional social histories are excellent, but they are still insufficient in number to support broad generalizations or models.⁷

Spain and in Europe. Although some aspects of the case made by Sánchez-Albornoz are persuasive, he has clearly dismissed too hastily the points that Castro raised. Castro, on the other hand, has tended to draw his evidence from what is too often a narrow literary base, slighting the legal evidence. The two historians have always studied the discipline from strongly contrasting points of view. Castro's orientation has been that of a philologist and a knowledgeable scholar of Spanish literature with a strong behaviorist attitude, while Sánchez-Albornoz has pursued an institutional approach; and they have disagreed sharply on the role played by Muslims and Mudejars in Spanish history.

⁶ Heath Dillard, "Women in Reconquest Castile: The Fueros of Sepúlveda and Cuenca," in Susan Mosher Stuard, ed., *Women in Medieval Society* (Philadelphia, 1976), 71–89; and Alberto García Ulecia, *Los factores de diferenciación entre las personas en los fueros de la Extremadura castellano-aragonesa* (Seville, 1975), 255–80, 297–322.

⁷ Robert I. Burns, S.J., has produced three studies of Valencia: *The Crusader Kingdom of Valencia*, 2 vols. (Cambridge, Mass., 1967), *Islam under the Crusaders* (Princeton, 1973), and *Medieval Colonialism*. Also see Esther Jirreno, *La población de Soria y su termino en 1270* (Madrid, 1958).



Figure 1: Ruins of the Roman bath at Segóbriga, Spain; the *caldarium* is in the foreground and the *tepidarium*, with the underground heating vent, in the background. Photograph by the author.

The major landmarks in the progress of municipal law, the *fueros* of Cuenca and Teruel, were not written until the closing decades of the twelfth century, and debate continues over the sources of the law in these charters and their respective influences upon one another.⁸ These codes also have much to say about social interaction between the conquering northern Christians and the Jewish and Muslim minorities of the south as well as about the position of women in the conquered towns of the frontier. But the social historian must utilize these *fueros* with great caution. The limitations of written law as a record of actual conduct are well known. Laws contain statements of ideal conduct as well as evidence of existing behavior. Within these limitations, the law codes are a valuable source for the study of the municipal bath-house as a significant social institution.

SOME FASCINATING REFERENCES to bathing in Christian Spain prior to the late twelfth century bath-house legislation appear in the records. The Arab commentator al-Hīmarī described the residents of Galicia in northwestern Spain as formidable warriors who bathed but once a year and then in cold water.⁹

⁸ James F. Powers, "Frontier Competition and Legal Creativity: A Castilian-Aragonese Case Study Based on Twelfth-Century Municipal Military Law," *Speculum*, 52 (1977): 483-87. For two recent Spanish surveys, see Alfonso García-Gallo, "Los fueros de Toledo," *AHDE*, 45 (1975): 407-58; and García Ulecia, *Los factores de diferenciación*, 357-452.

⁹ E. Lévi-Provençal, *La Péninsule Ibérique au Moyen Âge d'après le Kitāb ar-rawd al-Milār fi ḥabar al-aktār d'Ibn 'Abd Al-Mun 'im al-Hīmarī* (Leiden, 1938), 83; and Castro, *The Spaniards*, 272-73.

The *Primera crónica general*, moreover, recorded that Alfonso VI closed the Castilian baths, which he believed had corrupted his warriors and cost him the battle of Uclés and the life of his only son in 1108.¹⁰ But the *Primera crónica general* was compiled in the thirteenth century and may be a reflection of a much later point of view. There are references to the existence of baths in the *fueros* of Calatayud (1131), of Alfambra (1174–76), and of San Sebastián (undated, but possibly as early as 1153).¹¹ San Sebastián is on the northern coast of Spain in Navarre, hardly a center of Muslim civilization. And a fuller account of Christian bath-house usage survives in the lengthy charters awarded to the towns of Cuenca in Castile and Teruel in Aragón toward the end of the twelfth century. In establishing a Christian context for municipal bathing, Ruiz-Moreno and others have given insufficient attention to the evolution of the *fueros* and the correct place of Cuenca and Teruel in that evolution.¹² Their charters represent at least a century of legal development in municipal law. Once promulgated, these *fueros* became the formats and precedents for charters issued to a considerable number of towns in Castile and Aragón in the thirteenth century. Legal scholars commonly refer to related charters as a “family” and identify it by the name (town) given to the initial charter in the pattern. Through their families the Cuenca and Teruel charters profoundly influenced the development of municipal law on the peninsula. Another family of charters that emerged in this period was the Leonese-Portuguese group of the thirteenth century associated with the *fuero* of Coria. The charters of the Coria family also contain references to bath-house law.

The scholar who attempts to explore municipal problems in the twelfth and thirteenth centuries must possess an awareness of the *fuero* family patterns to arrive at any convincing conclusions. Muslim sources, moreover, are more numerous than Christian ones. As a consequence, the latter must be utilized with great caution to avoid the pitfalls traditionally associated with arguments from silence. The best sources from the Christian side are primarily legal. The twelfth century saw a European legal renaissance in municipal as well as other institutions. Iberia had a significant place in this renaissance, and the quality and length of municipal charters changed dramatically from the beginning to the end of the century.¹³ Even so, in literature and law the Christian states prior to 1200 bequeathed to historians far less in the way of

¹⁰ Ramón Menéndez Pidal, ed., *Primera crónica general de España que mandó componer Alfonso el Sabio y se continuaba bajo Sancho IV en 1289*, 2 (Madrid, 1955): 555; Castro, *The Spaniards*, 272–73; and Ruiz-Moreno, “Los baños públicos,” 152–53. Ruiz-Moreno has placed the reference in a somewhat incorrect context, an error Castro has not made. One must note, however, that the argument is double-edged; it could establish a case for Christian bathing in the eleventh century.

¹¹ José M. Ramos y Loscertales, ed., “Fuero concedido a Calatayud por Alfonso I de Aragón en 1131,” *AHDE*, 1 (1924): 411; José M. Lacarra and Angel J. Martín Duque, eds., “Fuero de San Sebastián,” in *Fueros de Navarra*, 1 (Pamplona, 1969): 270; and Manuel Albareda y Herrera, ed., *Fuero de Alfambra* (Madrid, 1925), 44.

¹² Powers, “Frontier Competition and Legal Creativity,” 471–87.

¹³ Even southern France with its advanced municipalities offers no comparable bathing legislation in the great thirteenth-century collections of *coutumes* of these towns. See Société Archéologique de Montpellier, *Thalamus parvus: Le petit thalamus de Montpellier* (Montpellier, 1840), and *Les coutumes de Perpignan, suivies des usages sur la dîme, des plus anciens privilèges de la ville, et des documents complémentaires* (Montpellier, 1848); and Ad. Tardif, ed., *Coutumes de Toulouse: Recueil de textes pour servir à l'enseignement de l'histoire du droit*, 2 (Paris, 1884).

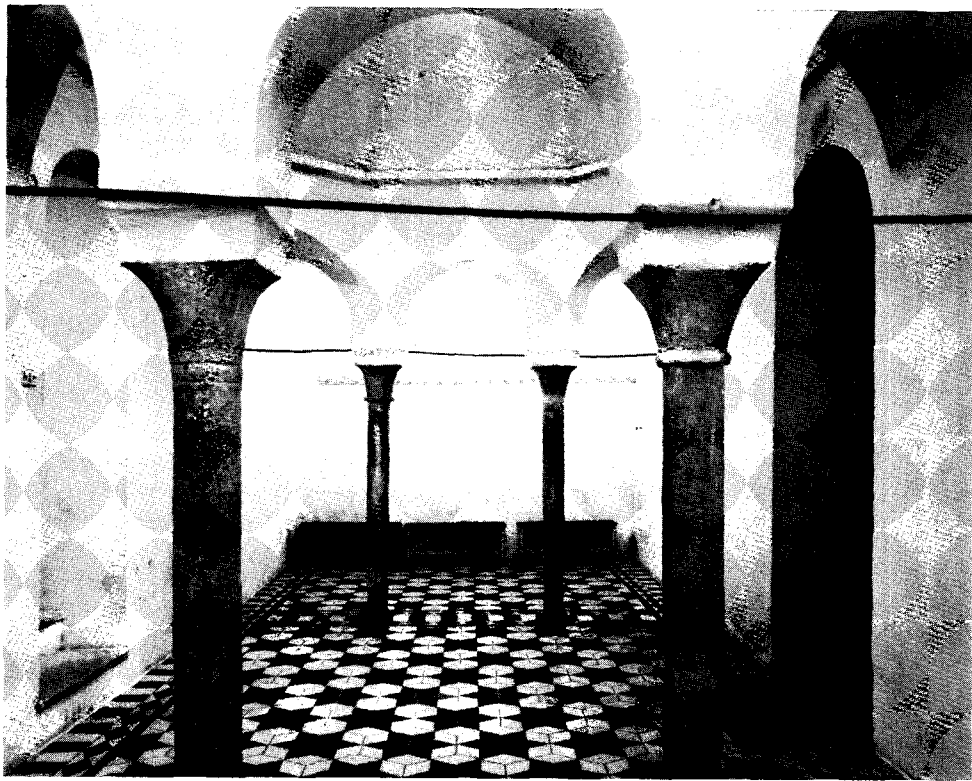


Figure 2: The steam room of the baths of 'Abd al-Mālik, Valencia. Photograph reproduced courtesy of Ampliaciones y Reproducciones Mas, Barcelona.

source materials than did the more advanced Muslim states. This comparative silence does not necessarily mean the absence of activity.

THE TYPICAL BATH-HOUSE Christians saw in Muslim towns was the descendant of the ancient Roman prototype in Iberia. Throughout the western Roman Empire the public baths (*thermae* and *balneae*), while varying considerably in scale, followed the same basic plan (see figure 1). A bather entered the *apodyterium* to undress and made his way to the *sudatorium*, a hot steam room to work up perspiration. From there he went to the *caldarium* to wash in a heated bronze basin and possibly to use one of the small private rooms that flanked the *caldarium*. Thus cleansed, he proceeded to the larger *tepidarium*, where the lightly heated larger chamber enabled him to cool gradually in preparation for a swim in the cold water pool of the last and largest chamber, the *frigidarium*. The medieval Muslim bath (*ḥammām*) was substantially different, for it had fewer chambers and concentrated more on perspiration as a method of cleansing. The dressing room was a place to disrobe, leave garments with the attendant, and receive bathing materials. The bather then proceeded in turn to an unheated room, a warm room, and finally a hot steam room, which combined the functions of the *sudatorium* and the *caldarium* (see figures 2 and 3). He then returned to the warm room, where epidermal waste

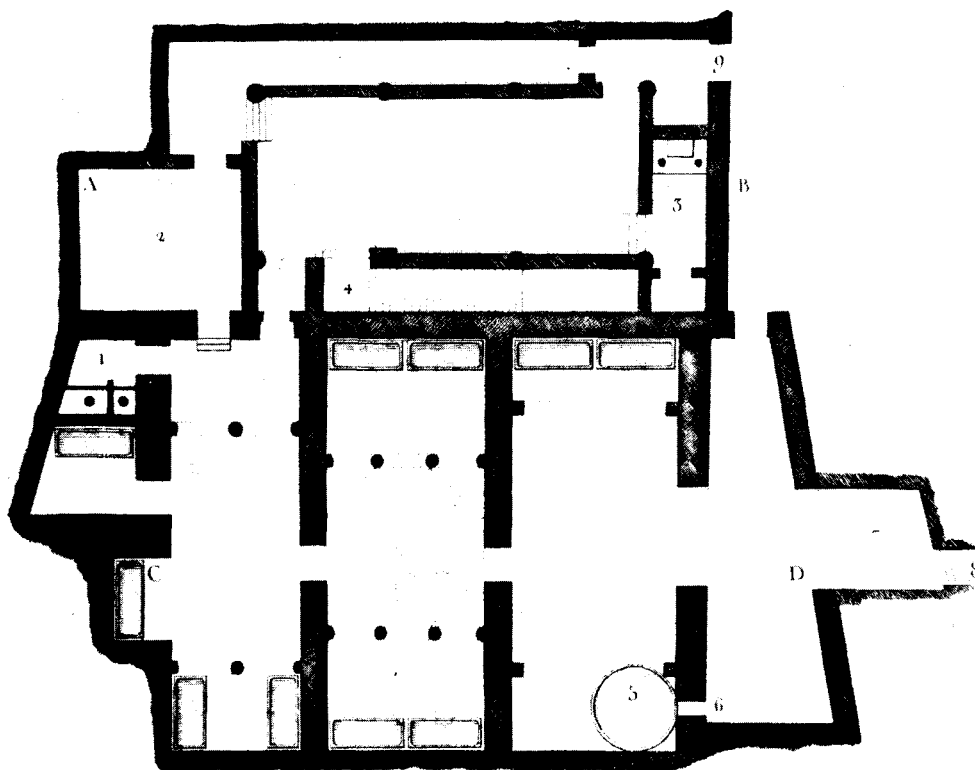


Figure 3: Ground plan of the bath of 'Abd al-Mālik, Valencia: the area between A and B is the reception, dressing, and cooling area; the area between C and D is the heated area; and the chamber labeled E is the steam room shown in figure 2. Photograph taken from Alexandre de Laborde, *Voyage pittoresque et historique de l'Espagne*, 1, pt. 2 (Paris, 1811): 78, and reproduced courtesy of Ampliaciones y Reproducciones Mas, Barcelona.

was removed by rubbing, scraping, and lathering and washing, followed by wrapping and a return to the dressing room and the entrance hall (see figure 4).¹⁴ In both the Roman and Muslim worlds, the larger and more elaborate baths provided a considerable variety of personal services, but the municipal charters seem to have regulated the simpler model. In Teruel and Albarracín the *almudazaf* (a municipal official charged with overseeing weights, measures, commercial rates, and public sanitation) had overall responsibility for the baths in the town.¹⁵ Each bath was controlled by a *señor del baño*, who could be

¹⁴ Jérôme Carcopino, *Daily Life in Ancient Rome*, ed. Henry T. Rowell and trans. E. O. Lorimer (New Haven, 1940), 254–63; J. P. V. D. Balsdon, *Life and Leisure in Ancient Rome* (New York, 1969), 26–32, and *Roman Women: Their History and Habits* (London, 1962), 265–70; J. Sourdél-Thomine, “Hammām,” in *Encyclopedia of Islam*, 3 (2d ed., Leiden and London, 1971): 139–44; and L. Torres Balbás, “Crónica arqueológica de la España musulmana, 30: El baño de Torres Torres y otros levantinos,” *Al-Andalus*, 17 (1952): 179–84. There was, in fact, no absolutely established order of bathing in a Roman bath-house. One could use the rooms in one’s own order of preference. The distinction between *thermae* and *balneae*, moreover, was not always clear: *thermae* was the later and more imperial term but not necessarily a sure indication of size; both were known in Rome and in the provinces. The household bath was referred to as a *lavatrina*. For the best account of Muslim bathing, see Ibn Sīnā’s “Canon of Medicine” in O. Cameron Gruner, *A Treatise on the Canon of Medicine of Avicenna, Incorporating a Translation of the First Book* (London, 1930), 232–39, 389–94.

¹⁵ Jaime Caruana Gómez de Barreda, ed., *El fuero latino de Teruel* (Teruel, 1974), 107; Max Gorosch, ed., *El fuero de Teruel* (Stockholm, 1950), 115; and Angel González Palencia and Inocenta González Palencia,

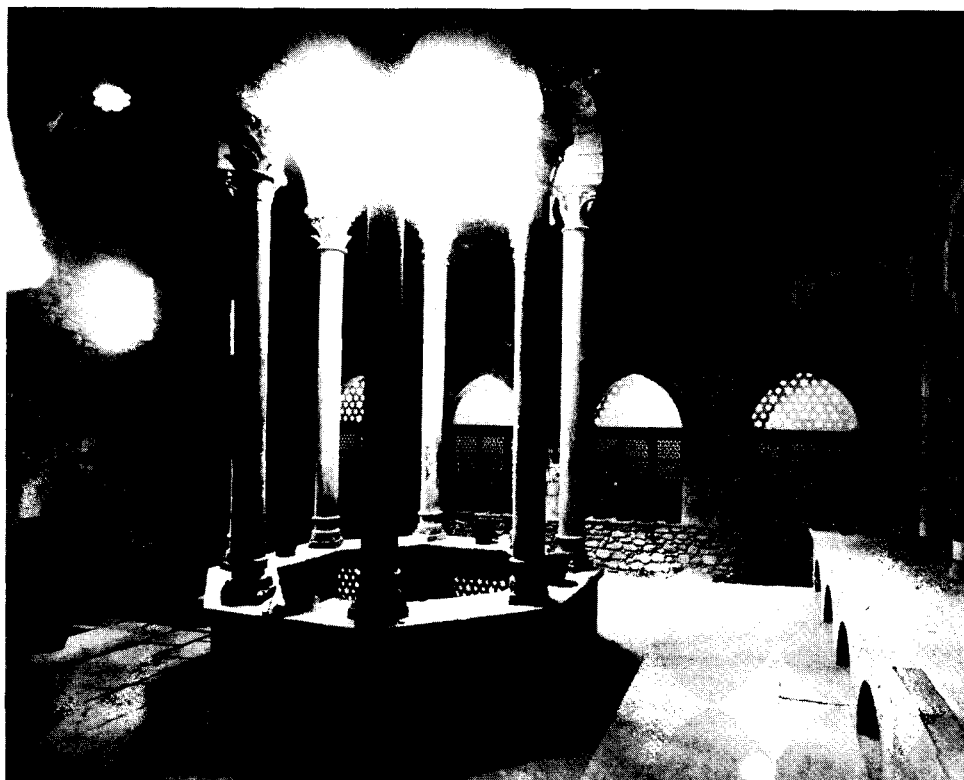


Figure 4: The bath at Gerona showing the reception and cooling area with the central air shaft. Photograph courtesy of Ampliaciones y Reproducciones Mas, Barcelona.

eds., "El fuero latino de Albarracín (fragmentos)," *AHDE*, 8 (1932): 428–29. The Castilian term is *almotacén* and the Catalan is *almudafà*, both derivative of the Arabic *muhtasib*. The Castilian *almotacén* seems to have lacked this specific charge in the Castilian towns, although he did collect the fines for bath-house mismanagement. Rafael de Ureña y Smenjaud, ed., *Fuero de Cuenca* (Madrid, 1935), 448–49; Jean Roudil, ed., *Les fueros d'Alcaraz et d'Alarcón*, 6 vols. (Paris, 1968), 6: 36–37, 392–93; "Fuero de Huete," Real Academia de la Historia, Madrid, 2-7-3, MS 37, f. 53; Rafael Ureña y Smenjaud, ed., *Fuero de Zorita de los Canes* (Madrid, 1911), 353; Jean Roudil, ed., *El fuero de Baeza* (The Hague, 1962), 429; Rafael Ureña y Smenjaud, ed., "Fuero de Iznatoraf," in *Fuero de Cuenca*, 424; and "Fuero de Alcázar," Biblioteca Nacional, Madrid, MS 11543, f. 65. The *fuero* of Plasencia shows payments for mismanagement going to the *mayordomos de concejo*, while the *fuero* of Sepúlveda assigns them to the *uez*; José Benavides Checa, ed., *El fuero de Plasencia* (Rome, 1896), 442; Emilio Sáez, ed., *Los fueros de Sepúlveda* (Segovia, 1953), 111; and Juan Gutiérrez Cuadrado, ed., *Fuero de Béjar* (Salamanca, 1975), 541, 543. (Once I have cited the *fueros* in their complete form, I will use the customary Spanish short-title form and place an "F" in front of the name of the place that received the charter.) The dates of these charters are as follows: Teruel, as late as 1196; Albarracín, 1212–20; Cuenca, 1189–90; Alcaraz, 1213; Alarcón, as late as 1214; Huete, as late as 1214; Zorita, 1218; Baeza, 1236; Heznatoraf, 1240; Alcázar, 1241; Plasencia, ca. 1212; Sepúlveda, 1300; and Béjar, the early 1290s. There is substantial literature on the *muhtasib*, whose authority over the baths probably derived from his responsibility for public sexual conduct, public health, and water. The Christian *mustasaf* inherited much of this authority in post-Reconquest towns, but his direct authority over the public bath-house was seldom specified. See C. Cahen and M. Talbi, "Hisba," in *Encyclopedia of Islam*, 485–89; Thomas F. Glick, "Muhtasib and Mustasaf: A Case Study of Institutional Diffusion," *Viator*, 2 (1971): 59–81; Pedro Chalmeta Gendrón, "La figura del almotacén en los fueros y su semejanza con el zabazoque hispanomusulmán," *Revista de la Universidad de Madrid*, 19 (1970): 145–67, and *El "señor del zoco" en España* (Madrid, 1973), 497–519; Francisco Roca Traver, *El mustaṣaf de Castellón y el "Libre de la mustaṣaffia"* (Castellón de la Plana, 1973), 32–35; Francisco Sevillano Colom, *Valencia urbana medieval a través del oficio de mustaṣaf* (Valencia, 1957), 24–30, 44–52; Hillgarth, *The Spanish Kingdoms, 1250–1410*, 80–82, 186–87; and Luis G. de Valdeavellano, *Curso de historia de las instituciones españolas de los orígenes al final de la Edad Media* (3d ed., Madrid, 1973), 546.

fined if he failed to provide for the needs of the bathers. Some charters specified those needs, particularly the requirement that the water be sufficiently hot or cold. The town of Heznatoraf also required bowls, buckets, and cloths; Plasencia added combs; and Tortosa insisted that the items supplied be not only clean but temperately warm.¹⁶ In most towns part of the fines collected for deficiencies in bath-house service were expended for the repair of the town's walls, and at Tortosa all revenues from the baths were devoted to that purpose.¹⁷

Until the charters of Cuenca and Teruel, the municipal *fueros* that mentioned baths concentrated largely on the questions of ownership and inheritance.¹⁸ No evidence of any difficulty in their management and operation appeared until the compilers of the codes of these two towns laid down the regulations that had widespread application in the thirteenth century. In addition, the new Christian settlers in Cuenca and Teruel utilized a social facility which had long been a mainstay in the daily life of the Muslims and Jews. That careful rules were needed regarding the sexes and their use of the baths will come as no surprise. During Roman times men and women customarily utilized the public baths simultaneously, though in separate facilities, until Hadrian decreed that different times of day be set aside for each sex.¹⁹ The Muslims also segregated the sexes in this way, and the municipal *fueros* directed Christians to follow the same tradition (see figure 5).²⁰ These laws offer insights into the position of women in this frontier society

¹⁶ *FCuenca*, 158–59; *FLat Teruel*, 291; *FTeruel*, 319; *FPlasencia*, 442; *FAlcaraz*, 2: 33; *FAlarcón*, 4: 55; “FHuete,” f. 10; *FZorita*, 46; *FBaeza*, 54; “FInzatoraf,” 54; “FAlcázar,” f. 14; D. Ramón Foguet and José Foguet Marsal, eds., *Código de las costumbres escritas de Tortosa* (Tortosa, 1912), 9:13:5; *FBéjar*, 70; *FSepúlveda*, 111; and Carlos Riba y García, ed., *Carta de población de la ciudad de Santa María de Albarracín* (Saragossa, 1915), 106–07. (This edition of the Romanceado version of the charter of Albarracín is inferior to the Latin edition of González Palencia and is only used when the fragments of the Latin edition lack a particular reference that the Romanceado has.) The *fuero* of Plasencia added an exemption from military service for this much harried official.

¹⁷ *FCuenca*, 448–49; *FLat Teruel*, 110; *FTeruel*, 117; *FAlcaraz*, 2: 393; “FHuete,” f. 53; *FZorita*, 353; “FAlbarracín,” 428–29; *FBaeza*, 429; “FInzatoraf,” 424; “FAlcázar,” f. 65; *Código de las costumbres escritas de Tortosa*, 11:115; and *FBéjar*, 543.

¹⁸ *FCalatayud*, 411; “FSan Sebastián,” 270; *FCuenca*, 158–61; *FPlasencia*, 14; *FAlcaraz*, 2: 3; *FAlarcón*, 7: 27; “FHuete,” f. 10; *Código de las costumbres escritas de Tortosa*, 9:13:5; *FBéjar*, 35; *FSepúlveda*, 25; and Manuel Dualde Serrano, ed., *Fori Antiquae Valentiae*, 160 vols. (Valencia, 1950; reprint ed., Madrid, 1967), 50: 2. For the newest and by far the most scholarly edition of the Valencian charters, see Germá Colon and Arcadi García, eds., *Furs de Valencia*, 2 vols. (Barcelona, 1970–). This work has not, however, reached the sections that deal with bath-house law.

¹⁹ “Hadrian,” 18, 10–11, in David Magie, trans., *Scriptores Historiae Augustae*, Loeb Classical Library, no. 1 (London, 1922), 56–57; and Balsdon, *Roman Women*, 224–25, 268–70, and *Life and Leisure in Ancient Rome*, 26–32. Some tawdry bath-houses permitted the mutual use of facilities, but they were regarded as a thin cover for prostitution.

²⁰ *FCuenca*, 158–61; *FLat Teruel*, 291; *FTeruel*, 319; José Maldonado y Fernández del Torco, eds., *El fuero de Coria* (Madrid, 1949), 118; *FPlasencia*, 438; *FAlcaraz*, 2: 32; *FAlarcón*, 7: 53; “FHuete,” ff. 9–10; *FZorita*, 43; “FAlbarracín,” 466; Juan Catalina García, ed., *El fuero de Brihuega* (Madrid, 1888), 162; Pedro Lumbreras Valiente, ed., *Los fueros municipales de Cáceres, su derecho público* (Cáceres, 1974), 125; Rafael Ureña y Smenjaud and Adolfo Bonilla y San Martín, eds., *Fuero de Usagre (siglo XIII) anotado con las variantes del de Cáceres* (Madrid, 1907), 127; “FInzatoraf,” 51; “FAlcázar,” f. 14; *Fori Antiquae Valentiae*, 134: 6; *FBéjar*, 67; and *FSepúlveda*, 111. The dates of the three Leonese charters are as follows: Coria, 1208–10; Cáceres, 1229–31; and Usagre, 1243. Brihuega dates from ca. 1223. The one exception that Ruiz-Moreno has found was Tortosa, a Catalan town at the mouth of the Ebro, which received its *costumbres* beginning in 1272; Ruiz-Moreno, “Los baños públicos,” 156. Here I think Ruiz-Moreno has let an ambiguous reference mislead



Figure 5: Women in a common bath, probably a resort mineral bath. Fifteenth-century copy of a manuscript attributed to Arnau de Vilanova, *Super Balneis Puteoli*, no. 2.396. *Catálogo de los Manuscritos existentes en la Biblioteca Universitaria de Valencia*, f. 4r; photograph by Luis López Remolino.

Although the usual pattern of sexual segregation was by days of the week, the charters were by no means consistent from town to town in the days assigned. Only Saturday was regularly given over to men, and depending on the town all other days could be for men or women or for minorities of either sex.²¹ Nor did the baths necessarily function on all days of the week. In the Aragonese towns of Teruel, Albarracín, and Valencia, it was illegal to heat

him into assuming a mixing of the sexes in the Tortosan bath-houses that is not justified by the full weight of the evidence, regardless of how interesting such an exception might be. First, no other charter fails to separate men and women in thirteenth-century Castile and the Crown of Aragón. Second, the bath law in the *costumbres* of Tortosa is very similar to that of Teruel and Albarracín, from which it may have derived; and the *fueros* of these towns segregated the sexes. Third, the Tortosan code merely gave both sexes the right to use the baths; it did not specifically permit simultaneous bathing by both sexes in the same facility. The context might also suggest that women bathe in the daytime and men at night: “E tots dies deuen esser apparellats per baynar tot hom e femna, que aquí’s vulla baynar, de nit e de dia”; *Código de las costumbres escritas de Tortosa*, 9:13:5.

²¹ *FCuenca*, 156–59; *FLat Teruel*, 291; *FTeruel*, 319; *FCoria*, 118; *FPlasencia*, 438; *FAlcaraz*, 2: 32; *FAlarcón*, 7: 53; “*FHuete*,” ff. 9–10; *FZorita*, 43; “*FAlbarracín*,” 466; *FBrihuega*, 162; *FCáceres*, 125; “*FIznatoraf*,” 51; “*FAlcázar*,” f. 14; *FUsagre*, 127; *Fori Antiquae Valentiae*, 134: 6; *FBéjar*, 67; and *FSepúlveda*, 111. There exists what certainly must be an error in the Benavides Checa edition of the *fuero* of Plasencia that suggests that both Christian women and Jews were assigned Friday in the bath-house. Such liberality and trust would have been unique to Plasencia among the Castilian frontier towns, and one rather suspects that women were assigned Thursdays instead, since this day goes unassigned in the charter.

(and therefore operate) the baths on Sunday, and the Castilian town of Sepúlveda also left Sunday unscheduled.²² At least one bath-house in Seville was open twenty-four hours—daytime reserved for women and the night for men. Tortosa's bath-houses were also open twenty-four hours, with no clear indication that men and women were segregated.²³ Substantial fines were assessed to men who either slipped into the bath-house on the days assigned to women or were caught spying through the windows of the structure.²⁴ The penalty for women found in a bath on a day assigned to men was more severe; they faced the loss of their legal rights as the wives and daughters of citizens.²⁵

The prohibitions and penalties in the charters reveal the dangers they were intended to prevent. Evidently, rape was a cause for concern, and the penalty for abusing women in the baths physically or even verbally, if proven, was death by hanging or by being cast down from a high cliff.²⁶ If committed outside the baths, verbal abuse was liable to a fine and possible exile, but, committed inside, it was a capital offense.²⁷ The law that permitted women to testify in *baño* violations applied only to wives and daughters of citizens and did not extend to nonresidents (for example, transients).²⁸ In Teruel and Albarracín, only men could decide disputes over which day women had the right to bathe.²⁹ The issue was important, since women had no legal protection if found in the bath-house on days assigned to men. Their legal status was reduced to that of prostitutes, and, like prostitutes, they could be abused with impunity.³⁰ What it meant to be labeled a prostitute under the law can be judged by other provisions of the charters. This much is clear, however. To be found in the bath-house on the wrong day was more than just physically dangerous for a respectable woman; she was threatened with a loss of status and descent into a social caste subject to heavy legal penalties.

The subject of theft in the *baño* received a good deal of attention in the charters. If severity of punishment is a criterion, the problem must have been

²² *FLat Teruel*, 291; *FTeruel*, 319; "FAlbarracín," 466; *Fori Antiquae Valentiae*, 134: 6; and *FBrihuega*, 162.

²³ Julio González, *Repartimiento de Sevilla*, 1 (Madrid, 1951): 522–24; and *Código de las costumbres escritas de Tortosa*, 9:13:5.

²⁴ *FCuenca*, 156–59; *FLat Teruel*, 291; *FTeruel*, 319; *FCoria*, 118; *FPlasencia*, 439; *FAlcaraz*, 2: 32; *FAlarcón*, 7: 53; "FHuete," f. 10; *FZorita*, 43; *FBrihuega*, 162; "FAlbarracín," 466; *FCáceres*, 125; "FIznatoraf," 51; "FAlcázar," f. 14; *FUsagre*, 127; *FBéjar*, 67; and *FSepúlveda*, 111. The fee was ten *maravedís* in the Cuenca family of charters and in Brihuega, one *maravedí* in Coria, Cáceres, and Usagre, and thirty *solidos* in Teruel and Albarracín.

²⁵ *FCuenca*, 156–59; *FPlasencia*, 439; *FAlcaraz*, 2: 32; *FAlarcón*, 53; "FHuete," f. 10; *FZorita*, 43; "FIznatoraf," 51; "FAlcázar," f. 14; *FBéjar*, 67; and *FSepúlveda*, 111. Teruel, Albarracín, Coria, Cáceres, Usagre, and Brihuega were content to charge women the same fine as men for this violation; *FLat Teruel*, 291; *FTeruel*, 3: 9; "FAlbarracín," 466; *FCoria*, 118; *FCáceres*, 125; *FUsagre*, 127; and *FBrihuega*, 162.

²⁶ *FCuenca*, 156–59; *FPlasencia*, 439; *FAlcaraz*, 2: 32; *FAlarcón*, 7: 53; "FHuete," f. 10; *FZorita*, 43; "FIznatoraf," 51; "FAlcázar," f. 14; *FBéjar*, 67; and *FSepúlveda*, 111.

²⁷ *FCuenca*, 320–21; *FPlasencia*, 70; *FAlcaraz*, 4: 29; *FAlarcón*, 7: 237; "FHuete," f. 37; *FZorita*, 253; *FBaeza*, 252; "FIznatoraf," 252; "FAlcázar," f. 42; *FBéjar*, 323; and Rafael Serra Ruiz, *Honor, honra, e injuria en el derecho meieval español* (Murcia, 1969), 66–67.

²⁸ *FCuenca*, 158–59; *FPlasencia*, 440; *FAlcaraz*, 2: 33; *FAlarcón*, 7: 54; "FHuete," f. 10; *FZorita*, 44; "FIznatoraf," 52; "FAlcázar," f. 14; *FBéjar*, 68; and *FSepúlveda*, 111.

²⁹ *FLat Teruel*, 291; *FTeruel*, 319; and "FAlbarracín," 466.

³⁰ *FCuenca*, 156–59; *FLat Teruel*, 369; *FTeruel*, 481; *FPlasencia*, 439; *FAlcaraz*, 2: 32; *FAlarcón*, 7: 53; "FHuete," f. 10; *FZorita*, 43; "FAlbarracín," 470; "FIznatoraf," 51; "FAlcázar," f. 14; *FBéjar*, 67; and *FSepúlveda*, 111. Also see Dillard, "Women in Reconquest Castile," 85–89.

acute. The law made a distinction between articles that belonged to the bath-house and those that belonged to bathers. The theft of a bath-house article cost the thief his or her ears. Worse still was the fate of the thief caught stealing a bather's personal possessions: loss of the ears for an article valued at up to ten *maravedís* or *mencales* and death by hanging or being thrown from a cliff for an article of greater value.³¹ Curiously, women's clothing was either excepted from this rule or dealt with in separate statutes. In Teruel and Albarracín, where the penalty was included among statutes on bath-houses, anyone who stole a woman's clothing while she bathed was liable to a fine of three hundred *sueños*, much higher than the twenty *sueños* involved in ordinary bath-house thefts. The Cuenca family of charters contained a similar provision, which is notable both because it was not included among the bath-house laws and because the fine was given in the Aragonese *sueños* rather than the *maravedí* coins collected in bath-house thefts.³² In both the Cuenca and Teruel families of *fueros*, the hapless prostitute could lose her clothing while bathing with no risk of fine to the thief.³³ The connection drawn between a woman's clothing and her status must have inspired the compilers of the *fuero* of Daroca in 1142 to place female thieves in the hands of a female jury and may also serve to explain a law in the *fuero* of Plasencia (ca. 1212) that classed prostitutes and thieves as a group and awarded the man who abandoned a prostitute her clothing.³⁴

Those who could afford them brought servants to assist them at the bath. The servants were admitted without charge; the *señor del baño* could, in fact, be fined for charging them.³⁵ In Coria, Usagre, and Cáceres a bather could instead bring a day hireling under the same exemption. City officials, who may have been tempted to impress the citizenry with the number of their servants, were limited specifically to one free servant per person.³⁶ By and

³¹ *FCuenca*, 158–61; *FLat Teruel*, 291; *FTeruel*, 320; *FPlasencia*, 443; *FAlcaraz*, 2: 33; *FAlarcón*, 7: 55; “FHuete,” f. 10; *FZorita*, 47; *FAlbarracín romanceado*, 107; *FBaeza*, 54; “FIznatoraf,” 54; “FAlcázar,” f. 14; *FBéjar*, 71; and *FSepúlveda*, 111. Zorita made the division at five *maravedís*, while Plasencia and Sepúlveda divided at two. The charters of Teruel and Albarracín mutilated the ears for up to twenty *sueños* in value but added only a lashing above that, an interesting contrast to the content of the Cuenca family.

³² Vicente Ramón Benedito, Fernando Álvarez Burgos, and Vicente Ramón Pérez, *La moneda medieval hispano cristiana* (Madrid, 1974), 25:02:01–15, 30:06:01–03; Filipe Mateu y Llopis, *Glosario hispánico de numismática* (Barcelona, 1946), 196–99; and Valdeavellano, *Curso de historia de las instituciones españolas*, 296–97.

³³ *FLat Teruel*, 291; *FTeruel*, 321; *FAlbarracín romanceado*, 107; *FCuenca*, 322–23; *FPlasencia*, 73; *FAlcaraz*, 4: 32; *FAlarcón*, 7: 240; “FHuete,” f. 37; *FZorita*, 256; *FBaeza*, 255; “FIznatoraf,” 254; “FAlcázar,” f. 42; *FBéjar*, 328; and Valdeavellano, *Curso de historia de las instituciones españolas*, 296–98.

³⁴ Rafael Esteban Abad, ed., “Fuero de Daroca,” in *Estudio histórico-político sobre la ciudad y comunidad de Daroca* (Teruel, 1959), 369; and *FPlasencia*, 681. Thieves and prostitutes were closely tied together in the Arab world, where prostitution was a legalized institution; see E. Lévi-Provençal, *España musulmana hasta la caída del Califato de Córdoba*, trans. Ramón Menéndez Pidal, vol. 5 of Pidal, ed., *Historia de España* (2d ed., Madrid, 1965), 289–90; Manuel Carboneres, *Picaronas y alcahuetes ó la mancebía de Valencia* (Valencia, 1876), 13–33; Ira M. Lapidus, *Muslim Cities in the Later Middle Ages* (Cambridge, Mass., 1967), 84–85, 270; and Leopoldo Piles Ros, *Estudio documental sobre el bayle general de Valencia, su autoridad y jurisdicción* (Valencia, 1970), 36, 124–25, 193, docs. 9, 12, 300.

³⁵ *FCuenca*, 156–59; *FLat Teruel*, 291; *FTeruel*, 319; *FPlasencia*, 438; *FAlcaraz*, 2: 32; *FAlarcón*, 7: 53; “FHuete,” f. 10; *FZorita*, 43; “FAlbarracín,” 466; *FAlbarracín romanceado*, 106; *FBrihuega*, 162; “FIznatoraf,” 51; “FAlcázar,” f. 14; *FBéjar*, 67; *FSepúlveda*, 111; and *Código de las costumbres escritas de Tortosa*, 1:115, 9:135. Only Teruel and Albarracín appear to have listed a fine for the *señor del baño*.

³⁶ *FCoria*, 118; *FCáceres*, 125; *FUsagre*, 127; and A. H. de Oliveira Marques, *Daily Life in Portugal in the Late Middle Ages*, trans. S. S. Wyatt (Madison, Wisc., 1971), 136–39. It was possible in these three towns to pledge the price of admission to the baths, provided that it was paid within nine days.

large, children were also admitted free if accompanied by parents. Tortosa seems to have limited this privilege to children who were seven years of age or less and who could be held by their mothers. This special exemption suggests some consideration was given to mothers insufficiently wealthy to have servants at home to care for their progeny while they bathed.³⁷

MUSLIM AND JEWISH BATH-HOUSES continued to operate independently of Christian regulations and usage in larger cities such as Seville and Valencia, which had numerous bath-houses. Mudejar and Jewish sources show, in fact, that baths of these minorities continued decades if not centuries after the Reconquest.³⁸ How, then, was Muslim and Jewish use of the bath-house modified by the impact of the Christian conquest? The smaller towns represented by the *fueros* under discussion lacked the numbers of citizens to support a large variety of *baños*, and in these instances the law appears to have dealt only with those bath-houses that Christians had the right to use. Among the towns governed by the Coria-style charters—the Leonese towns of Coria, Cáceres, and Usagre and the Portuguese towns of Alfaiates, Castel-Rodrigo, Castello-Melhor, and Castello-Bom—excluded Jews and Muslims from the baths that Christians used.³⁹ The Cuenca charters, by far the largest group covering towns from western to eastern Castile, permitted Jewish but *not* Muslim usage on Fridays and Sundays.⁴⁰ Although the *fueros* and other surviving source materials did not refer to them, there must have been other *baños* available to the excluded Jews and Muslims of the Coria- and Cuenca-style towns. Both religious groups required such facilities for religious purification and hygienic purposes. Apparently, Christians were not admitted to these baths, and the redactors of the *fueros* made no effort to legislate their usage. In Teruel and Albarracín, which possessed charters closely related but not identical to that of Cuenca, Jews and Muslims were permitted to use the Christian bath-houses, but on Fridays only.⁴¹ The matter of times allotted for the different sexes was apparently left for the minorities to work out at their own convenience. Finally, in the Catalan towns of Valencia and Tortosa, which had closer ties to Roman and Mediterranean civilization, Jews and Muslims were admitted to the same bath-houses without express limitations as to day and time.⁴² Noteworthy was the willingness to permit Muslim in

³⁷ *FCuenca*, 156–59; *F_{Lat} Teruel*, 291; *FTeruel*, 319; *FPlasencia*, 438; *FAlcaraz*, 2: 32; *FAlarcón*, 7: 53; “*FHuete*,” f. 10; *FZorita*, 43; “*FAlbarracín*,” 466; *FAlbarracín romanceado*, 106; “*F_{Iznatoraf}*,” 51; “*FAlcázar*,” f. 14; *FBéjar*, 67; *F_{Sepúlveda}*, 111; and *Código de las costumbres escritas de Tortosa*, 9:13:5.

³⁸ González, *Repartimiento de Sevilla*, 520–24; Burns, “Baths and Caravanserais,” 453–58; and Leopoldo Torres Balbás, *Ciudades hispano-musulmanas*, 1 (Madrid, 1971): 311–14.

³⁹ *FCoria*, 118; *FCáceres*, 125; *FUsagre*, 127; and Gregorio López, ed., *Las siete partidas del muy noble rey Don Alfonso el Sabio* (Madrid, 1844), 7:24:8. The *partidas* would seem to support this complete segregation.

⁴⁰ *FCuenca*, 156–57; *FPlasencia*, 438; *FAlcaraz*, 2: 32; *FAlarcón*, 7: 53; “*FHuete*,” f. 10; *FZorita*, 43; *FBrihuega*, 162; “*F_{Iznatoraf}*,” 50; “*FAlcázar*,” f. 14; *FBéjar*, 67; and *F_{Sepúlveda}*, 111. Brihuega, unlike the remainder of the Castilian towns, appears to have been closed on Sundays. Also see note 22, above.

⁴¹ *F_{Lat} Teruel*, 291; *FTeruel*, 319; and “*FAlbarracín*,” 466.

⁴² *Fori Antiquae Valentiae*, 134: 6; and *Código de las costumbres escritas de Tortosa*, 1:1:15. Valencia did close the baths on Sundays and on Good Friday. But there is no reason to assume, as Ruiz-Moreno casually has, that all minorities bathed together with Christians, for Tortosa’s charter made no such specification for men and women either. It is possible, of course, that the three religions as well as the two sexes did bathe in

addition to Jewish usage of the baths in the lands of the crown of Aragón, a privilege not granted in Castile. Not only does this Aragonese "permissiveness" point up one more important distinction between the two model charters of Teruel and Cuenca,⁴³ but it also raises questions as to whether the Aragonese were more tolerant in their municipal law.

The problem of violence and illegal mixed religious bathing was dealt with in the Cuenca family of *fueros* where Jews and Christians (but not Muslims) were permitted to share the *baño*. The *fuero* of Plasencia punished the individual who took arms into the bath-house with double fines, should anyone be wounded or killed as a result, but neither Plasencia nor any other town with a Cuencan charter could levy fines if violence erupted when Jews or Christians appeared on the wrong bathing day.⁴⁴ The bathers were evidently left to solve this problem on their own, although the Jewish minority was often exposed thereby to the greater danger in the Christian-dominated towns. The compilers of Teruel and Albarracín listed a fine of thirty *súeldos* for Jews and Muslims who entered on the incorrect day but did not give a fine for Christians making the same mistake.⁴⁵ No fine was imposed for violence done to bath-house equipment, except for damage to the wheel that fed water into the bath-house. Damage to a *rueda de baño* was assessed at ten *maravedis* in Castile and thirty *súeldos* in Aragón.⁴⁶

Since the body of law governing the social functioning of the bath-house is peculiar to the Castilian and Aragonese charters, it is difficult to establish the degree of Muslim tradition operative in its creation. The chief sources for Muslim social history that treat the bath-house are the *ḥisba* tracts, a special body of advisory materials written for the office of the *muḥtasib*.⁴⁷ Contained in these are certain guidelines for bath-house supervision. But the *ḥisba* do not concern all of the matters addressed by the laws of the municipal charters. They stress, for example, the importance of personal hygiene and the avoidance of accidental pollution of the baths, including regulations for covering wash basins to prevent spillage as well as for personal cleanliness of bath attendants and the laundering of their clothing.⁴⁸ A late ninth-century Andalusian *ḥisba* prohibited women from entering the baths after being ill or

common in these two Catalan coastal towns, but the thrust of the evidence from all of the other Castilian and Aragonese municipalities makes this unlikely, at least for the thirteenth century.

⁴³ See note 2, above.

⁴⁴ *FCuenca*, 158–59; *FPlasencia*, 43, 145, 441; *FAlcaraz*, 2: 33; *FAlarcón*, 7: 55; "FHuete," f. 10; *FZorita*, 45; "FIznatoraf," 53; "FAlcázar," f. 14; *FBéjar*, 69; and *FSepúlveda*, 111.

⁴⁵ *FLat Teruel*, 291; *FTeruel*, 319; and "FAlbarracín," 466.

⁴⁶ *FCuenca*, 242–43; *FLat Teruel*, 295; *FTeruel*, 349; *FPlasencia*, 602; *FAlcaraz*, 3: 53; *FAlarcón*, 7: 156; "FHuete," f. 23; *FZorita*, 166; *FAlbarracín romanceado*, 115–16; *FBaeza*, 163; "FIznatoraf," 163; "FAlcázar," f. 28; Galo Sánchez, ed., "Fuero de Soria," in *Fueros castellanos de Soria y Alcalá de Henares* (Madrid, 1919), 252; and *FBéjar*, 202.

⁴⁷ Although many of the *ḥisba* tracts remain unedited, some edited versions exist with translations into European languages. Several of these are attributed to residents of the Iberian Peninsula, and the *ḥisba* in general show a marked similarity of tradition throughout the Muslim world in the Middle Ages. Cahen and Talbi, "Hisba," 485–89; and Chalmeta Gendró, *El "señor del zoco" en España*, 370, 373, 423, 438, 448, 452.

⁴⁸ E. Lévi-Provençal, *Séville musulmane au début du XII^e siècle: Le traité d'Ibn 'Abdun sur la vie urbaine et les corps de métiers* (Paris, 1947), 152; and Pedro Chalmeta Gendró, "El 'Kitāb fī ādāb al-ḥisba' (Libro del buen gobierno del zoco) de al-Saqāṭī," *Al-Andalus*, 33 (1968): 158. For the use of baths for medical examinations and emergencies, see Chalmeta Gendró, "El 'Kitāb fī ādāb al-ḥisba' . . . de al-Saqāṭī," 54, 124.

recently giving birth. The offense was compared to a man entering the bath-house without a loin-cloth,⁴⁹ for there was a demand both inside and outside of Al-Andalus for the proper covering of the body.⁵⁰ Since the *muhtasib* was also charged with public conduct and dress, tradition provided considerable support for his work to maintain propriety inside and outside of the bath-house. A twelfth-century Andalusian *hisba* by Ibn 'Abdun reveals a bit of Victorian-like sensitivity in his suggestion that the bath fee collector move away from the bath-house entrance on ladies' day, lest the establishment assume the appearance of a house of prostitution.⁵¹ Twelfth-century North African Muwaḥḥid puritanism is even more starkly revealed in the case of a wealthy resident of Fez who was compelled to tear down a new section of his house in the face of accusations that from its elevated pavilion he was spying on the women disrobing in the nearby bath of Bint al-Bāz.⁵² Clearly, these Muslim mores reinforced Christian attitudes toward women in the *baño*.⁵³ But there is something more. Christian law, more stringent than Muslim law, suggests that Christians lacked confidence in handling the types of social situations that might have arisen in the bath-house and, moreover, that women had a somewhat different institutional position in terms of their legal rights in Christian Spain than did Muslim women in Muslim Spain.

THUS, THE NATURE OF THE CHRISTIAN ENCOUNTER with the urban bath-house of southern Iberia was complex. Although bath-houses were utilized, a remark-

⁴⁹ Yahyā Ibn 'Umar, "Akhāmas-sūq," in Emilio García Gómez, ed. and trans., "Unas 'ordenanzas del Zoco' del siglo IX: Traducción del más antiguo antecedente de los tratados andaluces de *hisba*, por un autor anda'uz," *Al-Andalus*, 22 (1957): 31, 55. Given the context, the "illness" suggested may be connected with menstruation and the overall concern with ritual purification.

⁵⁰ Al-nās.r il-haqq, Zaydī Imām of Amul (ca. A.D. 917), "A Zaidī Manual of *Ihsba* of the 3rd Century (H)," in R. B. Sergeant, ed., *Revista degli Studi Orientali*, 28 (1953): 9; and G. M. Wickens, ed., "Al-Jarsīfī on the *Ihsba*," *Islamic Quarterly*, 3 (1956): 180. Al-Jarsīfī composed a late thirteenth-century *hisba*.

⁵¹ Lévi-Provençal, *Seville musulmane au début du XII^e siècle: Le traité d'Ibn 'Abdun*, 155.

⁵² Ibn Abī Zar', *Rawd al-qutās*, trans. Ambrosio Huici Miranda, 1 (2d ed., Valencia, 1964): 130-31. The destruction took place in 1192.

⁵³ Scholars might hope to find useful transitional material for Muslim and Christian responses to the *baño* in the experience of the Mozarabic minority (those who maintained their Christian faith while under Muslim rule). A considerable amount of documentary evidence from this group survives, especially at Toledo where a Mozarabic population persisted until the Christian conquest of that city in 1085. This group retained its cultural identity in the centuries that followed the reunification with their fellow Christians from the north. Bath-houses also survived the Christian occupation and were operating at least as late as the thirteenth century, a conditioning process at Toledo that probably affected the kind of law written later at Cuenca and Teruel and doubtless helped forestall the outright rejection of the *baño* in Christian society. See Angel González Palencia, *Los mozárabes de Toledo en los siglos XII y XIII*, 4 vols. (Madrid, 1930), vol. prelim.: 54-56; and García Gallo, *Fueros de Toledo*, 407-58. But little of the functioning of this process is visible in the surviving Mozarabic sources of the period. The continued existence of the *baño de Caballero* and the *baño de Yaix* (both in the cathedral *barrio*, the latter owned by the archbishop) is verified by a number of documents that mention *baños* in the twelfth and thirteenth centuries, but little more than their location is noted. González Palencia, *Los mozárabes de Toledo*, 1: 56-57, 259, 288-89, 2: 58, 85-86, 98-99, 150-51, 190-91, 3: 353-54, 514, 524-25. The documentation is quite thin for the office of the *almotacén*, who sometimes oversaw the baths, and makes no reference to public bathing. References to Mozarabs who have "bath-keeper" (*bañero* and *hamami*) as part of their family names also survive. *Ibid.*, vol. prelim.: 225, 236, 1: 178-79, 2: 50, 150-51, 190-91. A detailed description of a mason's contract to reconstruct the baths owned by the *convento* of San Clemente offers valuable information on construction materials but few indications of actual operation. *Ibid.*, 3: 330-32. As a result, only the barest inferences remain regarding the functioning of the Mozarabs as intermediaries between northern Christian and Muslim in this critical area.

ably complex body of regional laws was deemed necessary to regulate their use. These laws furnish insights into the mentality and the social structure of the Christian conquerors and reveal glimpses of the lives of medieval women in the Castilian and Aragonese frontier towns. The relative scarcity of women and their role as maintainers of familial wealth and prestige through their ability to inherit lands and titles gave them an advantageous position in comparison with their peers north of the Pyrenees.⁵⁴ But the bath-house, for all of its attractiveness, threatened them and their elevated but highly encompassed status in very fundamental ways. The fines assessed for spying and male entry on the incorrect day do not seem to have been a sufficient guarantee of female security. Hence, the need to punish severely the theft of women's clothing in the *baño* and the tendency to apply the death penalty as a sanction against physical or verbal assault upon them. The lawmakers' efforts represented an intrusion of the law-making mentality of the era into a realm where blood feuds would otherwise have prevailed. The same frontier situation that gave women their enhanced position as creators of the family and holders of property bound them ever more tightly to male concerns.⁵⁵ Men remained for women their chief security, their greatest threat, and—if need be—their civic avengers. The laws governing the use of bath-houses provide one clear indication that women continued to remain very much in the control of men, a control intensified by the exigencies of the frontier. If a woman fell from this lofty position of legal security, her value to the dominant Christian male society was so compromised that she was stripped of all legal rights and reduced to the status of a sex object.

The revelations of bath-house law concerning the treatment of Jews and Muslims in the towns are equally significant. Although it is necessary to be cautious in drawing general conclusions where regional variations may have existed, this much seems certain. The concept of segregating religions in the *baño* was an original Christian contribution to bath-house law. No evidence exists inside or outside of Spain for the Muslim imposition of religious segregation in a public bath. Yet no Castilian law code specifically permitted Muslims to utilize the same *baño* as Christians, and significant limitations were placed upon Jewish access to the facilities. Although the towns under the crown of Aragón appear to have been somewhat more open, the tradition of the upland towns of Teruel and Albarracín (the closest to Castile geographically, legally, and economically) merely permitted Muslims to bathe within the same limited periods as Jews in the Christian baths.

Such restrictions add a particular note of irony to Américo Castro's assertion that the baths provided a prime example of *convivencia* (living together) in daily life. Perhaps these laws instead reveal the constraints within which that *convivencia* operated: the coexistence of mutually exclusive groups that had to conform to standards established by the dominant Christians but agreed to by

⁵⁴ Dillard, "Women in Reconquest Castile," 71-89; and García Ulecia, *Los factores de diferenciación*, 233-80, 297-322.

⁵⁵ Dillard, "Women in Reconquest Castile," 86-89.

the subject minorities. To some degree, even Christian women conformed to this same pattern of minority status. This tension of the minority-majority relationship was not likely to remain static. Either the three religions and their attendant subcultures were destined to interact increasingly freely and blend their respective societies more closely or the relationship was likely to deteriorate. The prospects for an acculturation process that could integrate these societies to a greater degree turned upon the opportunities to encourage cultural diffusion through contact.⁶⁶ Laws providing for segregated use of bath-houses indicate that at the outset substantial restrictions were placed on this kind of contact. The frontier environment that created this special opportunity for acculturation also generated the hostilities and suspicions that inhibited its success. Indeed, the laws also suggest the violence that lay just below the surface in a recently conquered town, awaiting perhaps only a confrontation in the *baño* to kindle old antagonisms into flame once more. Yet some degree of acculturation clearly took place.⁶⁷ Municipal bath-house laws show that legislators attempted to provide a situation in which the various elements that composed urban society in the Spain of the Reconquest might coexist at least to the extent of sharing, if only on a segregated basis, a personal public facility. This tentative coexistence can be argued from the chronicles and literary sources of the later Middle Ages, which indicate a continuity of bath-house usage without exceptional incidents of violence. To this extent the municipal laws for the *baños* were not unworkable; indeed, they assisted in sustaining a restrained *convivencia* of town life in post-Reconquest Iberia. Unfortunately, limited cultural diffusion permitted a protocolonial mentality to dominate over the cosmopolitan.

This unique coexistence of the three religions finally ended in the sixteenth century with the persecution and expulsion of first the Jewish and then the Muslim minorities. As Christian relations with the peninsular minorities deteriorated, municipal bathing persisted although it apparently decreased in significance. In Seville the number of bath-houses gradually declined in the later Middle Ages, although at least two survived the expulsions of the sixteenth and early seventeenth centuries.⁶⁸ At least one Mudejar bath-house was built in Játiva as late as the fifteenth century, and there were fourteenth- and fifteenth-century references to baths in Murcia.⁶⁹ No detailed examination of this decline in peninsular public bathing has yet been undertaken,

⁶⁶ For an excellent introduction to the process of acculturation and its application to these Christian municipalities, see Thomas F. Glick and Oriol Pi-Sunyer, "Acculturation as an Explanatory Concept in Spanish History," *Comparative Studies in Society and History*, 9 (1969): 136-54. For a more detailed application of this concept to the thirteenth-century Valencian setting, see Robert I. Burns, "Journey from Islam: Incipient Cultural Transition in the Conquered Kingdom of Valencia (1240-1280)," *Speculum*, 35 (1960): 337-56, and "Spanish Islam in Transition: Acculturative Survival and Its Price in the Christian Kingdom of Valencia, 1240-1280," in Speros Vryonis, ed., *Islam and Cultural Change in the Middle Ages* (Wiesbaden, 1975), 87-105. Also see Burns, *Islam under the Crusaders*.

⁶⁷ Glick and Pi-Sunyer, "Acculturation as an Explanatory Concept in Spanish History," 151-54.

⁶⁸ González, *Repartimiento de Sevilla*, 520-24.

⁶⁹ L. Torres Balbás, "Crónica arqueológica de la España, 42: Játiva y los restos del palacio de Pinohermosó," *Al-Andalus*, 23 (1958): 154-55; and Juan Torres Fontes, *Colección de documentos para la historia del Reino de Murcia*, vol. 1: *Documentos de Alfonso X el Sabio* (Murcia, 1963), 60-63.

although some general explanations have been offered. The Black Death doubtless contributed to decline of the *baños* by its decimation of the population and the increased threat of the disease in public places. One recent study has seen the fifteenth century as the age in which opposition to public bathing grew while the bath-house crumbled under charges of immorality. It was, moreover, a time that witnessed a decline in the standards of personal cleanliness and an increase in anti-Mudejar attitudes.⁶⁰ In Granada bath-houses were torn down within fifteen years after its conquest in 1492, and their end was accelerated by the revolt of the native Mudejars in 1525. Though the



Figure 6: Seal of Córdoba, undated but believed to be from the thirteenth century; the *albolafia* (water wheel) is located in the lower left. A reproduction of the original seal done by the Servicio de la Reproducción de Sellos, Archivo Histórico Nacional de Madrid, and photographed by Joel Villa.

baths still attracted Christian patronage, the excuses for closing them offered at the time included new allegations that men and women were bathing together and old accusations, made first in the ancient *Primera crónica general*, that the baths debilitated men needed for war.⁶¹ The closings were clearly related to the drive on the part of the monarchy to extirpate Muslim practices

⁶⁰ Hillgarth, *The Spanish Kingdoms, 1250–1410*, 190. Yet a Catalan traveler, Pero Tafur, could still register some surprise at mixed public bathing in Basel and Bruges: “The bathing of men and women together they take to be as honest as church-going with us”; Tafur, *Travels and Adventures, 1435–1439*, ed. and trans. Malcolm Letts (London, 1926), 185, 200.

⁶¹ Torres Balbás, *Ciudades hispano-musulmanas*, 312; Luis de Mármol Carvajal, “Historia del rebelión y castigo de los moriscos del Reino de Granada,” in *Biblioteca de autores españoles desde la formación del lenguaje hasta nuestros días*, vol. 21: *Historiadores de sucesos particulares* (Madrid, 1858), 161–62, 164; and Castro, *The Spaniards*, 273. Also see note 10, above.

among the Moriscos.⁶² Indeed, the hostility created by the Catholic Kings' renewal of the Reconquest in the fifteenth century has often been seen as the primary cause for the end of the medieval atmosphere of *convivencia*.

THE ERA OF BATH-HOUSE *convivencia* came to its symbolic close during the ten-year campaign that reduced the Islamic Kingdom of Granada. King Ferdinand and Queen Isabella employed Córdoba, the former Muslim capital, as their base of operations, and Isabella ordered the *albolafia* or water-raising device to be dismantled because its creaking disturbed her sleep. This *albolafia*, which appears on the medieval seals of the city of Córdoba (see figure 6), carried the waters of the Guadalquivir River across the Arco de Sillares to the Torre de Baño, feeding the baths of the Almohad emirs and the gardens of Alfonso XI, facilities the queen was then enjoying.⁶³ The execution of Isabella's order marked the end of one era and the beginning of another—an era in which the *limpieza de sangre* (purity of blood) was deemed more important than bodily cleanliness. In a world constrained by the narrow intolerance of the *Reyes Católicos*, the medieval bath-house, with the limited societal interaction that it might have fostered, had clearly lost its former stature. But, in the retrospect of the three centuries that preceded the queen's gesture, the act seems to symbolize the end of a process of deterioration in relationships rather than its beginning.

⁶² Hillgarth, *The Spanish Kingdoms, 1250–1410*, 190.

⁶³ Adolfo Herrera, "Sello de Córdoba de mediados del siglo XIV," in *Colección de documentos inéditos para la historia de España*, 109 (Madrid, 1894): 249–50.

Marriage Politics and the Family in Florence: The Strozzi-Medici Alliance of 1508

MELISSA MERIAM BULLARD

FROM THE TIME OF ITS EARLIEST CHRONICLERS scholars have recounted the history of Florence in terms of the histories of its great patrician families. When we think of Florence—whether of its politics, its society, its economy, or its artistic monuments—the names of such families as the Medici, Strozzi, Bardi, Guicciardini, Pitti, and Rucellai spring to mind. Florentines themselves were very aware of the importance of family ties and connections; for this reason, families often kept detailed lists, called *prioristi*, which recorded the names of those members who had held public office, among their private papers. Both Leon Battista Alberti and Francesco Guicciardini wrote long treatises on the family, and in the early sixteenth century Lorenzo Strozzi went so far as to compose a compendium of lives of famous members of his family that he hoped would serve as an example to later generations of Strozzi.¹ The family was the basic social unit in Florence, serving as the intermediary between the individual and his surrounding world.² For the Florentine of the Renaissance, birth largely determined social status and, to a lesser degree, political posture and economic position as well.

Marriage, unlike birth, offered some choice and was, therefore, perhaps the single most important means of improving social status in a society with limited access to the ranks of the patriciate. Since marriage also provided the vehicle to perpetuate the family, a betrothal was never just an arrangement between two individuals but between two families: matrimony had too many lasting effects to be left to lovers or to romance. Florentines negotiated their marriages carefully and formalized arrangements with a written legal contract as in any ordinary business transaction. Marriages were usually arranged through the mediation of friends or relatives who acted as marriage brokers. Because successful betrothals brought prestige to the brokers, Florentines often recorded not only the marriages that took place in their own families but also those betrothals they helped arrange for others. Marriage brokering formed part of the patronage system, and clients frequently relied

¹ Leon Battista Alberti, *I libri della famiglia*, ed. R. Romano and A. Tenenti (Turin, 1969); Francesco Guicciardini, "Memorie de famiglie," in R. Palmarocchi, ed., *Scritti autobiografici e rari* (Bari, 1936), 3–50; and Lorenzo Strozzi, *Le vite degli uomini illustri della casa Strozzi* (Florence, 1892).

² Lauro Martines, *The Social World of the Florentine Humanists* (Princeton, 1963), 50.

upon the good will and services of more powerful friends to act in their behalf.³

In Renaissance Florence social status and political power were inextricably linked, and, thus, marriage also had political consequences. Those in authority sometimes intervened to prevent two families from uniting through marriage if the proposed union seemed to threaten the stability of the government. After the failure of the Pazzi Conspiracy, Lorenzo the Magnificent prevented the Pazzi daughters from marrying according to their rank,⁴ and the alliance between Lorenzo's granddaughter Clarice de' Medici and Filippo Strozzi in 1508 very nearly foundered for the same reason. Apart from such extreme cases involving a threat to the security of the state, marriages served in general to solidify alliances between families, and intermarriage was one way of consolidating the social, economic, and political interests of the Florentine aristocracy. A Medici quite naturally married a Tornabuoni, a Salviati, or a Ridolfi. In like manner, Filippo Strozzi and his brothers and sisters married into the leading Florentine houses of the Medici, Soderini, Capponi, Rucellai, Ridolfi, and Nasi.⁵ In 1503 Neri Capponi emphasized the importance and prevalence of marital alliances within the Florentine aristocracy to Lorenzo Strozzi on the occasion of his son's betrothal to Lorenzo's sister, Caterina: "God has seen fit to give both of us that joy that we desire. Even though this engagement [between my son Gino and your sister Caterina] brings us no closer tie since our families are already so intimately related, first through my mother who was born among you [the Strozzi] and then through Niccolò [Capponi, who married your sister Alessandra], still it is a bond between us reaching beyond the intimate friendship we had with your father."⁶

³ The Medici correspondence reveals that they were besieged with requests for help in arranging many marriages; Mediceo avanti il Principato [hereafter MAP], Archivio di Stato, Florence [hereafter ASF]. Filippo Strozzi made one such request on behalf of Simone Ridolfi, who wished to arrange a marriage between his daughter and Pierfrancesco Borgherini, in a letter to Lorenzo de' Medici, by then Filippo's brother-in-law. The letter demonstrates not only that the Medici's influence was considered indispensable in negotiating the match but also how closely political patronage and family affairs were bound together. Filippo Strozzi to Lorenzo de' Medici, April 5, 1514, ASF, MAP, 108, f. 131: "Simone Ridolfi ha desiderio maritare una sua figlia a Pierfrancesco Borgherini secondo mi ha riferito Filippo suo figliuolo si trova qui, e perché senza lo aiuto vostro non si confidono ottenere nulla, desidererebbono Vostra Magnificenza mi dessi commessione ne parlassi per parte di quella con detto Pierfrancesco quando non vi sia molesto. Haro charo potere spendere il nome vostro, che oltre allo essere figliuolo di Simone, sviscerato servitore della casa vostra, anchora a me è nipote et quando da Vostra Magnificenza ne abbia ordine verso tale commessione con quella modestia solita procedere in simil maneggi." In his treatise on the family, Leon Battista Alberti openly counselled a man to seek three attributes in his prospective wife: beauty, family, and wealth; *I libri della famiglia*, 132.

⁴ Bartolomeo Cerretani, *Istoria fiorentina*, Biblioteca Nazionale, Florence [hereafter BNF], MS II, III, 76, f. 321. Rinaldo degli Albizzi's three granddaughters most likely suffered a similar fate when, after his exile following Cosimo de' Medici's triumphant return to Florence in 1534, they took the veil and never married; Julius Kirschner and Anthony Molho, "The Dowry Fund and the Marriage Market in Early Quattrocento Florence," *Journal of Modern History*, 50 (1978): 425-26.

⁵ Pompeo Litta, ed., *Le famiglie celebri italiane* (Milan, 1819-1902), fasc. XLIX, dispensa 68, tav. 18.

⁶ Neri Capponi to Lorenzo Strozzi, January 2, 1503, ASF, Carte Stroziane [hereafter CS], 3d ser., 145, f. 95: "Et a voi et a noi piaccia adio di darcene quel gaudio che desideriamo et benché fra noi non sia grande accrescimento di parentado per essere stretti fra noi prima per nostra madre nata di voi e dipoi per Niccolò, pure aremo confirmatione fra noi oltre all'amicitia stretta tenavamo con vostro padre." In his important recent study of the Florentine family, Francis William Kent has referred to part of this letter but has incorrectly identified Lorenzo Strozzi as the bride's uncle rather than as her brother; F. W. Kent, *Household and Lineage in Renaissance Florence* (Princeton, 1977), 96.

The marriage between Filippo Strozzi and Clarice de' Medici took place in 1508. In its own right the Strozzi-Medici match—documented in what for the Renaissance is an unusually extensive set of public records, contemporary chronicles, and private Strozzi papers—makes an interesting case study of the political, social, and economic consequences of Florentine patrician marriages. But beyond its intrinsic importance the marriage casts light on that tumultuous and complex period of the early sixteenth century when Piero Soderini headed the republic. The marriage became a *cause célèbre* on which the whole city of Florence took sides. The ensuing crisis exposed a fatal weakness in the governing coalition that led to its downfall and overthrow in 1512, when the exiled Medici returned to Florence by force. And this marriage, particularly the reactions it elicited from among the Strozzi kin, presents a good test case with which to evaluate recent divergence of scholarly opinion about the relative significance of small and large family group ties in early modern European society. The present study also attempts to augment recent investigations of the structure of premodern families by focusing on the dynamics of kinship.⁷

IN TERMS OF THE GENERAL PATTERN of Florentine aristocratic marriages, the betrothal of Clarice de' Medici and Filippo Strozzi, an alliance between two of the richest and most powerful patrician families in Florence, was not unusual. But, when considered beyond the social standing of the couple, the marriage did not conform to expected patterns, and, given the political ties and traditions of both families, the match was highly unlikely. Clarice was the granddaughter of Lorenzo the Magnificent, who from 1469 through 1492 had been Florence's first citizen and its virtual ruler. Although Filippo also descended from one of the oldest and richest Florentine families, his family had been among the bitterest political rivals of the Medici in the fifteenth century. In 1434 the Strozzi, together with other members of the vanquished Albizzi faction, had been driven into exile and ostracized from political office by Clarice's great-great-grandfather, Cosimo de' Medici. Interpreting literally the old saw about politics and strange bedfellows, however, provides a clue to

⁷ For the best statement of present scholarship on the family in medieval and Renaissance society, see Jacques Heers, *Le clan familial au moyen âge* (Paris, 1974). Francis William Kent has reviewed the Florentine part of the issue and the relevant bibliography; see his *Household and Lineage in Renaissance Florence*, 3–15. I generally agree with Kent's and Robert Wheaton's perception that terminology used to designate various degrees of kinship relations needs to be more precise; see their respective Reviews of Heers's *Le clan familial au moyen âge* in the *Journal of Family History*, 2 (1977): 77–86; and *Speculum*, 52 (1977): 378–80. When I refer to Filippo Strozzi's wider family group, I mean his agnatic kin group—that is, his paternal kinsmen, the members of the Strozzi lineage. I have found it difficult, however, always to use their recommended terminology borrowed from anthropologists; terms such as “descent group,” “patrilineage,” and “agnatic kin group” define fixed structural relationships and thus inadequately express the fluid and dynamic side of family life in Renaissance Florence. Kent has substituted the more precise “lineage” for the often confusing “extended family” and “family clan,” but “lineage” connotes a hard-to-shake image of the family as a vertical line of descendants over a period of generations and does not fully convey a sense of the family made up at any given moment of living, interacting members. Wheaton has acknowledged that Heers's “blurring of these differences [between various types of kin groups] recognizes the social reality,” and he has also pointed out the pressing need for studies of “kinship in action” to flesh out our understanding of premodern family groups; Review of Heers's *Le clan familial au moyen âge*, 739–80.

understanding the origins of the apparent misalliance of the Medici and Strozzi and its consequences for the future of Florence.

When the Medici were in undisputed control of Florence and the Strozzi were political undesirables, such a marriage was never—and would not have been—considered by either family; but by 1508 the political situation in Florence had radically changed. In 1494 the Medici government of Piero de' Medici (Lorenzo's son and Clarice's father) had been overthrown, the Medici exiled, and a republican government instituted. The new government drew strength from the election of Piero Soderini as *gonfaloniere* (chief executive) with life tenure in 1502. The possibility of the Medici's return to Florence seemed even more remote when Piero de' Medici died in 1503, leaving behind as his only heirs two small children, Clarice and her brother Lorenzo, who were living in Rome with relatives of their mother, Alfonsina Orsini. Other members of Piero de' Medici's immediate family were his two brothers, Giuliano and Cardinal Giovanni de' Medici, neither of whom had lifted a finger against the republican government in Florence. By 1508, however, opposition to the government of Piero Soderini was growing among certain aristocrats. Some of them looked back with nostalgia to the days of Lorenzo the Magnificent's rule, when members of their families had had a share in the rewards deriving from their participation in government, and were not at all opposed to the idea of the Medici's coming back to Florence as private citizens.⁸ Among those aristocrats who opposed Soderini for both personal and political reasons was Bernardo Rucellai, a close friend and adviser to Filippo Strozzi's widowed mother, Selvaggia, and father-in-law of Filippo's brother, Lorenzo. When the time came, Bernardo Rucellai wielded powerful influence in Strozzi family councils, advising Selvaggia to conclude the engagement for her son with the Medici.

Aware of the growing support for the Medici inside Florence, Cardinal de' Medici, who was then at the Vatican, began to entertain thoughts of his family's return to its native city; to that end he courted the favor of members of the large Florentine community in Rome.⁹ A critical dimension of the Medici's plan for their return was a marriage arrangement for Clarice in Florence. Such a union offered two distinct benefits. First, by marrying into a prominent Florentine aristocratic family, she would gain further support for the Medici among the patricians opposed to the Soderini government. Second, her marriage would provide a foothold in Florence for her brother Lorenzo, who was the only grandson of Lorenzo the Magnificent and, after his uncles, sole male heir to the Medici's claims to the city. The Medici could easily have arranged an excellent betrothal for Clarice in Rome through the good connections of her uncle, Cardinal de' Medici, and her mother's Orsini

⁸ Francesco Guicciardini, *Storie fiorentine dal 1378 al 1509*, ed. R. Palmarocchi (Bari, 1931), 323; Filippo de' Nerli, *Commentari de' fatti civili occorsi dentro la città di Firenze dall'anno 1512 al 1537*, 1 (Trieste, 1839): 158–62, 168–72; and Rosemary Devonshire Jones, *Francesco Vettori, Florentine Citizen and Medici Servant* (London, 1972), 55–59.

⁹ Guicciardini, *Storie fiorentine*, 323; and Bartolomeo Cerretani, *Dialogo della Mutazione*, BNF, Magliabechiana, II, t. 106, f. 147.



Figure 1: One of the famous wrought-iron lanterns—the work of Caparro—adorning the Strozzi Palace in Florence. Photograph taken from Georgina Mason, *Italian Villas and Palaces* (London, 1959), and reproduced courtesy of Thames and Hudson, Ltd., publishers.

relatives. But such a marriage would not have promoted the family's bid to return to its native city.

Negotiating an advantageous marriage in Florence was no easy matter. The stigma of exile attached to the Medici name and the fear of political reprisals from the Soderini government were enough to deter many potential in-laws. Had the Medici not been exiles, throngs of suitors would have flocked to Clarice, drawn by the handsome dowry of six thousand florins that Cardinal de' Medici had promised her. The Medici's initial attempt to negotiate a match with a member of the Pitti family in 1506 miscarried after Soderini had the prospective groom's father summoned before the judicial council and forced him to deny the arrangement. An effort to marry Clarice to Soderini's own nephew also met with failure.¹⁰ Only on the third try did the Medici

¹⁰ Guicciardini, *Storie fiorentine*, 325, 326.



Figure 22 Inner courtyard of the Strozzi Palace; begun in 1489 by Benedetto da Maiano for Filippo the Elder, the palace was not completed until 1536. Photograph taken from Mason, *Italian Villas and Palaces*, and reproduced courtesy of Thames and Hudson, Ltd., publishers.

approach Filippo Strozzi, who, although the fatherless third son of a family traditionally opposed to the Medici, was still one of the wealthiest and most eligible Florentine bachelors.

From Filippo and his mother Selvaggia's point of view, the prospective *parentado* (betrothal) to Clarice offered several advantages. The alliance would protect them from a repetition of past political abuse should the Medici ever return to power—a prospect not to be discounted in view of the popularity and power of Cardinal de' Medici and his elevated position in the Church. Perhaps the most persuasive family influence in this direction came from Bernardo Rucellai. Although once an intimate of Lorenzo the Magnificent, Bernardo had become estranged from Piero de' Medici. But, after the election of Soderini, whom Bernardo bitterly and openly opposed, he joined that group of aristocrats who favored the Medici and welcomed their return.¹¹

¹¹ Cerretani, *Istoria fiorentina*, f. 321; Guicciardini, *Storie fiorentine*, 283, 327; and Nerli, *Commentari de' fatti civili occorsi dentro la città di Firenze*, 160.

Accordingly, Bernardo encouraged Selvaggia to consider a marriage with the Medici for Filippo. In addition to their close connections with these members of the pro-Medici and anti-Soderini factions in Florence, Selvaggia and her son had a personal grievance against the *gonfaloniere* over the settlement of a lawsuit in which he had exerted his considerable influence against them.¹² And, finally, according to Lorenzo Strozzi, Selvaggia was on the lookout for a large dowry for Filippo to augment the family coffers, badly depleted by the construction of the Strozzi Palace. Clarice's dowry of six thousand florins was highly attractive.¹³

Filippo was undoubtedly aware of the grave political risks involved in a *parentado* with Clarice, and only after months of secret negotiations through the mediation of Dominican friars did he agree to sign a marriage contract in July 1508. Only then did Selvaggia and Filippo send a close family friend, Michelagnolo Biscioni of Santa Maria Impruneta, to Rome to carry Filippo's agreement to the Medici and complete the arrangements.¹⁴ In the contract, written in his own hand, Filippo promised to marry Clarice within eight months or pay a two-thousand-ducat penalty for noncompliance. The Medici were bound by the same fine, should they break the contract.¹⁵ The arrangement was kept secret even from other Strozzi, and, to protect himself against early disclosure, Filippo left Florence in mid-September in the company of his mother ostensibly on a pilgrimage to Loreto. From there he continued on alone to Naples. According to the plan, Filippo was to go on to Rome after a decent interval and pretend to have become so infatuated with Clarice (and with the size of her dowry) that he betrothed her on the spot.¹⁶ While Filippo was still in Naples, however, rumor of the *parentado* prematurely leaked out in Florence and touched off a furor in the city.

The news of Filippo's engagement created such a scandal in Florence that the whole city down to its meanest citizens gossiped about nothing else.¹⁷ Before long the matter became a volatile political issue, a *caso di stato*, which polarized the city—supporters of Soderini and opponents of the marriage

¹² The lawsuit involved a dispute with Alfonso Strozzi over the settlement of Filippo the Elder's estate and the financial obligation of Filippo the Younger, Lorenzo, and Alfonso to complete the Strozzi Palace. Alfonso received preferential treatment because of his friendship with the *gonfaloniere*; Lorenzo Strozzi, *Vita di Filippo Strozzi*, in G.-B. Niccolini, *Filippo Strozzi, tragedia* (Florence, 1847), xii.

¹³ Strozzi, *Vita di Filippo Strozzi*, xii. Selvaggia was Filippo the Elder's second wife and mother of two of his three sons, Lorenzo and Filippo; their elder half-brother Alfonso was born to their father's first wife. For the financial circumstances of Selvaggia and her two sons following the death of Filippo the Elder, see Richard Goldthwaite, *Private Wealth in Renaissance Florence: A Study of Four Families* (Princeton, 1968), 78–92, tables 7–10.

¹⁴ Strozzi, *Vita di Filippo Strozzi*, xii, xiv, xvii; Guicciardini, *Storie fiorentine*, 326; and Filippo Strozzi to Antonfrancesco degli Albizzi, December 22, 1508, ASF, CS, 3d ser., 134, f. 51.

¹⁵ For the conditions of the agreement, see Filippo Strozzi to his brother Lorenzo, December 9, 1508, ASF, CS, 3d ser., 134, f. 50: "Io mi sono obligato per scripta di mia propria mano in fra termini di mesi 8 cominciando da luglio, se bene mi ricorda, dovere dare l'anello a detta sposa, e non lo facendo s'intenda essere tenuto alla pena di ducati o dua o tre mila che questo anchora non so apunto. Vedi memoria ho, et io ho anchora loro obligho nella medesima forma con le conventioni istese."

¹⁶ Strozzi, *Vita di Filippo Strozzi*, xiv. Also see the documents in ASF, CS, 5th ser., 87, unnumbered *Ricordo* entries for September 13, 14, 1508.

¹⁷ Strozzi, *Vita di Filippo Strozzi*, xv–xvi; and Giovambattista Ciei to Giovanni Strozzi, "Duolmi che per tutta la terra non si parlla d'altro," January 11, 1509, ASF, CS, 3d ser., 134, f. 53. Also see Giovambattista Ciei to Giovanni Strozzi, December 16, 1508, ASF, CS, 3d ser., 134, f. 52.

versus opponents of Soderini and supporters of the match. The betrothal became a test of Soderini's political strength, because he was determined to use it to combat the *Palleschi*, or Medici sympathizers, and rally popular support to himself, whereas his opponents were equally determined not to let the *gonfaloniere* manipulate the case for his own designs.

From the beginning Soderini suspected that Filippo had not acted alone in arranging his engagement with Clarice, and he interpreted the *parentado* politically, as part of a larger scheme by his *Palleschi* opponents to overthrow the government and reinstate the Medici. He had the issue brought up before the highest governmental council, the *Signoria*. On December 7, 1508, the *Signoria* ordered Filippo to appear before it by December 25 or be exiled for ten years in Naples. In a second decree the *Signoria* forbade his mother and brothers (on pain of a fine of ten thousand ducats) to provide him with any financial or material aid.¹⁸ Soderini then began to investigate the leading *Palleschi* and other opponents of the government for the instigators of the betrothal. Ultimately, twelve citizens were charged before the *Otto di Guardia*, the magistracy that prosecuted criminal matters, as perpetrators of the *parentado*, and others were subsequently implicated as sympathizers of the match.¹⁹ At the head of the list—and publicly associated with the affair—were Bernardo Rucellai and his sons, Palla and Giovanni. Giovambattista Ciei's letter to Giovanni Strozzi, who was in Ferrara, indicates how strongly popular sentiment was running against Rucellai: "Bernardo is not worthy of mention, and we will grieve more at his return [from Venice] than at his departure, because he who betrays his fatherland does not deserve to be a part of it."²⁰

These events occurred in December 1508 at the same time as the elections

¹⁸ For the two decrees, see ASF, Signori e Collegi, Deliberazioni, Ordinaria Autorità, 110, ff. 124v-25: "Et deliberaverunt precipi Filippo alterius Filippi Mattei de Strozis civi florentino quatenus usque per totam diem vigesimam quintam presentis mensis decembris debeat se personaliter presentasse coram dictis magnificis dominis ad parendum eorum mandatis aliter intelligatur esse et sit confinatus in regno neapolitano per decennium continuum proxime futurum a die sue ibidem facte representationis ad qua confinia se immediate representare debeat personaliter et dicta confinia observari debent sub pena rebellionis et confiscationis omnium suorum bonorum." Guicciardini said that both decrees passed the *Signoria* with nine black beans; *Storie fiorentine*, 328. Also see Giovambattista Ciei to Giovanni Strozzi, December 16, 1508, ASF, C.S., 3d ser., 134, f. 52; Strozzi, *Vita di Filippo Strozzi*, xvi; Cerretani, *Istoria fiorentina*, f. 321; and Giovanni Cambi, *Istorie fiorentine*, in Idelfonso di San Luigi, ed., *Delizie degli eruditi toscani*, 21 (Florence, 1786): 222.

¹⁹ Jacopo Pitti, "Apologia de' Cappucci," *Archivio storico italiano*, 4 (1853): 313; Nerli, *Commentari de' fatti civili occorsi dentro la città di Firenze*, 160; and Guicciardini, *Storie fiorentine*, 328, 331. Lucrezia de' Medici, sister of Cardinal de' Medici, and her husband, Jacopo Salviati, a rich and powerful aristocrat who had grown disenchanted with Soderini, were also mentioned prominently in connection with the engagement; Guicciardini, *Storie fiorentine*, 326-27, 330.

²⁰ Giovambattista Ciei to Giovanni Strozzi, January 11, 1509, ASF, C.S., 3d ser., 134, f. 53: "Di Bernardo non merita parlarne, e della sua tornata ci dorremo più che della partita, che chi fa contro alla patria non merita d'esserne. . . ." To counter this widespread suspicion and clear himself, Rucellai felt compelled to address the *Signoria*. In a letter of December 31, 1508, he appealed to his past record of loyalty to the city; see ASF, Signori, Carteggi, Responsive Originali, 31, f. 253: "Credo sia assai noto nel tempo che vixi Lorenzo de' Medici che sempre mi sforzai che lui usassi bene le grandezze sue e su la morte non manco per me di ridurre le cose al bene in tanto che incorsi in grave pericolo e mio figliuolo Cosimo ne fu facto rebelle. E di poi ne' casi del frate [Savonarola] fui el primo che persuade largamente la quiete e la pace." This letter verifies Guicciardini's account, which mentions that Rucellai was particularly blamed for the *parentado* and that, in consequence, he wrote from Venice in his own defense; *Storie fiorentine*, 331. Guicciardini also reported that Bernardo's son, Giovanni Rucellai, was rumored to have made several secret trips to Rome; *ibid.*, 325.

for the major councils in the city government. Soderini tried desperately to use the betrothal not just to combat his opponents but also to increase the size of his own party in the elections. In an inflammatory speech before the Great Council preceding the selection of the new *Signoria*, he denounced the marriage as a threat to the security of the state and implied that it was part of a Medici plot to overthrow the popular government.²¹ Malicious reports surfaced, accusing Filippo of flagrant disrespect for the government. One rumor, obviously designed to arouse passions, is recorded in two contemporary, slightly variant accounts. Filippo had allegedly written to his brothers Alfonso and Lorenzo from Naples that the government's negative reaction to his marriage was hardly surprising nor was it of much concern to him since the city was run by *foggettini*, an upper-class term of scorn for the masses.²² Public sentiment rose to such a fevered pitch over this story that a series of anonymous accusations were lodged against Filippo on December 12, 1508, and January 2, 15, and 16, 1509, in the *tamburo* of the *Otto di Guardia* for criminal prosecution.²³

Soderini's efforts to manipulate the situation backfired. He faced a rising tide of angry opposition, which made clear, even to the *gonfaloniere*, the extent to which his support among the patriciate and his control over the government had declined. At first, Filippo's sympathizers were reluctant to rise to his defense for fear of angering Soderini or of being labeled *Palleschi*; they preferred to wait and gauge the strength of Soderini's forces before taking action. But soon a growing number of citizens rallied to Filippo's cause—not just Medici sympathizers, but opponents of Soderini, friends and clients of the Strozzi, and some who merely sympathized with Filippo because of Soderini's autocratic handling of the case. The *gonfaloniere* appeared to be mistreating Clarice in precisely the same way that Lorenzo the Magnificent had mistreated the daughters of the Pazzi family after the Pazzi Conspiracy—by preventing their marriages to be arranged at suitable rank. The rumor spread that Soderini wanted to pass a law prohibiting Clarice from marrying in Florence at all; if this were the case, his opponents asserted, Soderini himself should be punished for permitting his nephew to be considered as a candidate for Clarice's hand.²⁴ Even Pope Julius II intervened to defend Filippo's *parentado*. Little sympathy flowed between Julius and Soderini, and the pope had always favored Cardinal de' Medici, who probably induced him to influence the situation in Florence. The pope sent two briefs to the archbishop of Florence, one of which was presented to the *Signoria* on December 23.²⁵ Julius told the *Signoria* not to impede but to favor the *parentado*. He claimed

²¹ Guicciardini, *Storie fiorentine*, 329.

²² Cerretani, *Istoria fiorentina*, f. 321; and Guicciardini, *Storie fiorentine*, 328.

²³ ASF, *Otto di Guardia e Balìa, Partiti e Deliberazioni*, 143, f. 23; Guicciardini, *Storie fiorentine*, 329; Nerli, *Commentari de' fatti civili occorsi dentro la città di Firenze*, 161; Cerretani, *Istoria fiorentina*, f. 321; and Strozzi, *Vita di Filippo Strozzi*, xvi.

²⁴ Cerretani, *Istoria fiorentina*, f. 322, and *Dialogo della Mutazione*, f. 147; and Guicciardini, *Storie fiorentine*, 330–32.

²⁵ ASF, *Signori, Carteggi, Missive*, 56, f. 128. Filippo mentioned in a letter to Antonfrancesco degli Albizzi on December 22 that the letters from the pope had been sent by "a friend"; ASF, CS, 3d ser., 134, f. 51.

that Clarice, fatherless and still a child, was under his special protection. Again, the precedent of the Pazzi daughters was invoked, since in 1478 Pope Sixtus IV had tried the same tactic.²⁶ Soderini angrily replied to Julius by way of the Florentine ambassador in Rome, telling the pope to mind his own business.²⁷

Far more ominously, Soderini had even less success in the councils of state. Two years earlier he had encountered no opposition in halting the proposed Pitti-Medici marriage when the Pitti were denounced before the *Quaranta*, or judicial board, but now even the elections went against him. Among the new members of the *Signoria* was Neri Capponi, a Strozzi in-law sympathetic to Filippo.²⁸ When at the end of December Filippo returned from Naples to answer the summons of the retiring *Signoria*, half of its members refused to vote for Soderini's recommendation to bar Strozzi from Florentine territory.²⁹ At the hearing charges against him were deferred until the case had been heard by the *Otto di Guardia*, which meant that final jurisdiction would pass into the hands of the new *Signoria*. Since the new *Signoria* was unlikely to cooperate, the only hope remaining to the opponents of the marriage lay in a trial before the *Otto di Guardia*.³⁰ Filippo had been in some danger that the *Otto di Guardia* would transfer jurisdiction of his case to the dreaded *Quaranta*, which functioned like a committee of public safety and which, because of its larger size and more popular character, would undoubtedly have rendered a stiffer sentence.³¹ Fresh charges were lodged against Filippo in the *tamburo* to assure continued prosecution of the case.³² Fortunately for him, though prob-

²⁶ For the contents of Julius's letter as reported in the *Signoria's* response to Roberto Acciaiuoli, Florence's ambassador to Rome, on December 23, 1508, see ASF, Signori, Carteggi, Missive, 56, f. 128. Also see Cerretani, *Istoria fiorentina*, f. 321.

²⁷ Soderini's response to Julius demonstrates the extent to which the *gonfaloniere* saw the *parentado* as a threat to the Florentine state; see ASF, Signori, Carteggi, Missive, 56, f. 128: "... et questo è, che importando questa cosa alla universale quietà et pace di questa città et non potendo partorire qui se non disordine et scandolo noi teniamo per certo se Sua S.^{ta} havessi intera notitia di tucte le circumstantie et mali effecti che ne può nascere, mai harebbe voluto con dispiacere et mala contentezza di ogni homo di questa città ricercare una cosa dalla quale non può seguire se non male ... che tractandosi in questo *parentado* della libertà et unito vivere di questa repubblica non è da credere che tanti cittadini che ci hanno dentro particolare interesse fussino per comportarlo et non mancherà al Rev.^{mo} Cardinale de Medici atteso tanto sue buone qualità allogare la sua nipote in qualche altro luogo honorevolmente."

²⁸ Strozzi, *Vita di Filippo Strozzi*, xix; and Guicciardini, *Storie fiorentine*, 329-30. The new *Signori* were Neri di Gino Capponi, Raffaello di Alfonso Pitti, Averado Peruzzi, Federigo di Giuliano Gondi, Gentile Sassetti, Ugolino di Giuliano Mazzinghi, Biagio Monti, and Girolamo dello Straffa.

²⁹ For confirmation of his appearance on December 25, see ASF, Signori e Collegi, Deliberazioni, Ordinaria Autorità, 110, f. 125. The *Signori* who voted against Soderini were Francesco di Bartolomeo Pandolfini, Antonio di Lione Castellani, Luigi Guicciardini, and Francesco Calderini. Also see Guicciardini, *Storie fiorentine*, 330; Strozzi, *Vita di Filippo Strozzi*, xix; and Cerretani, *Istoria fiorentina*, f. 321.

³⁰ Later, in fact, on January 23, the *Signoria* voted to drop all charges against Filippo, his mother, and his brothers; ASF, Signori e Collegi, Deliberazioni, Ordinaria Autorità, 111, f. 8.

³¹ Giovambattista Ciei to Giovanni Strozzi in Ferrara, January 11, 1509, ASF, CS, 3d ser., 134, f. 53: "La querela pende agli Otto e bisogna corrino certi termini come delle chause civili e a 17 di questo la debbono terminare o mandarla al giudizio della Quarantia che sarebbe pericholosa materia per Filippo, e per lui si farebbe gli Otto la terminassino loro e ne fa lui e chi l'a messo in questa impresa ogni sforzo." For a good characterization of the political make-up of the *Quaranta*, which because of its larger size was less susceptible to pressure from Filippo's supporters, see Nerli, *Commentari de' fatti civili occorsi dentro la città di Firenze*, 161: "... purché in tanto largo numero potevano gli uomini più liberamente giudicare senza rispetto de' gran parentadi de' capi delle sette."

³² The *tamburo* was the special receptacle for the lodging of anonymous denunciations against citizens; see Guicciardini, *Storie fiorentine*, 329.

ably by design, the original charges with the *Otto di Guardia* on December 12 had barely missed the final day for adjudication by the old *Otto di Guardia* and had to be held over for the new magistrates to be seated in January.³³ As in the case of the new *Signoria*, Soderini was much less sure of the loyalty of the new members than he had been of the old.

The new *Otto di Guardia* ordered Filippo to appear on January 5 and again on January 12, 1509. The charges accused him of breaking the law by making a contract with exiles.³⁴ Clarice's late father, Piero de' Medici, had been declared an exile in 1494, and his three armed forays against the republic afterwards placed all of his offspring under the ban of exile *in perpetuo*. The legal precedent for this charge reached back to 1393, when the law was enacted against the Alberti family under *gonfaloniere* Maso degli Albizzi.³⁵ Filippo skillfully defended himself by arguing that the charge against him was applied *ex post facto*. He admitted to contracting the *parentado* with Clarice but said that it could no longer be annulled, since he was already married in the eyes of the law; if he had only known beforehand that such a *parentado* would displease any individual, much less the *Otto di Guardia*, he would not have committed himself. To the charge that he had trafficked with exiles, he responded that technically he had had no dealings with any exiles whatsoever; he had only dealt with Dominican friars and with Alfonsina Orsini, Cardinal de' Medici, and Giulio de' Medici, who, though barred from Florentine territory, had never been formally exiled. Appealing to more recent legal precedent than the statute under which he had been charged, he reminded the *Otto di Guardia* of a series of laws that exempted the daughters of exiles from the restrictions placed on their fathers. Two of the three statutes, passed in 1451 and 1458 under the Medici, specified that whoever married the daughter of an exile was subject only to confiscation of property and confinement outside the city. The third, passed in 1494, stated that such a person should at most pay a fine of four thousand *lire*.³⁶ Filippo then produced examples of marriages in the Pazzi and Martelli families that had taken place under similar circumstances. He claimed that he had been moved solely by the good qualities of the girl herself, not by any desire to disturb the peace and restore the Medici. He was first and foremost a Strozzi, he declared, and could never forget the injustices done to his family by the Medici; colonies of exiled Strozzi—stretching from Ferrara, Mantua, and Padua to Provence and Avignon—were constant reminders of the political persecution his family had suffered at their hands. Finally, he noted that the magistrates should encourage rather than forbid his marriage to Clarice because, should the Medici try

³³ For confirmation of Ciei's letter of January 11, 1509 that Filippo's supporters were doing their best to keep the case out of the *Quaranta*, see Cerretani, *Istoria fiorentina*, f. 321; for Ciei's letter, see note 31, above.

³⁴ The charges are reported in the text of the sentence imposed; see ASF, *Otto di Guardia e Balìa, Partiti e Deliberazioni*, 143, ff. 23–25. For a published version, see Pasquale Villari, *Niccolò Machiavelli e i suoi tempi*, 2 (Milan, 1927): 548–52. Also see Strozzi, *Vita di Filippo Strozzi*, xx; and Guicciardini, *Storie fiorentine*, 329.

³⁵ Pitti, "Apologia de' Cappucci," 315.

³⁶ Cerretani cited the statutes; see *Istoria fiorentina*, f. 321. At the rate of 7 *lire* to the florin, the fine equalled about 570 florins. Also see Cambi, *Istorie fiorentine*, 222.

to re-enter Florence, they would find him the very last in-law they could turn to for help.³⁷

The actual sentencing by the *Otto di Guardia* did not occur until January 16, again missing by just one day the deadline for the case to have been transferred to the *Quaranta*. The magistrates voted unanimously to uphold the more recent laws exempting daughters from the penalties imposed on their exiled fathers. The final sentence was relatively light, permitting Filippo to marry Clarice but fining him five hundred gold florins.³⁸ The council also ordered that Filippo be confined in the Kingdom of Naples for three years, but, in fact, before the year was out he was allowed to return to Florence.³⁹

Soderini had been powerless to prevent the marriage or even to use the issue to strengthen his sagging forces. The evidence suggests that—once the election went against him, the old *Signoria* acted against his wishes, and the *Otto di Guardia* failed to allow the case to be transferred to the *Quaranta*—Soderini recognized the inevitable and moved toward the face-saving measure of having the case settled by the *Otto di Guardia* with a mild sentence.⁴⁰ The basic implications of the Strozzi affair both for Soderini's control of the government and for the life of the republic itself were indeed ominous. Soderini had put his reputation on the line and failed. Although probably insufficient in itself to unseat the *gonfaloniere*, Filippo's betrothal provided a convenient issue on which citizens could take sides, and it serves as a barometer to measure how much support Soderini had lost as early as 1508 and how much the fear and hatred of the Medici had diminished in the fourteen years since their exile. There were other indicators that Soderini was losing his grip, but none so open to the public eye. He was unable, for example, to prevent several less sensational political marriages, nor could he stop the Medici's candidate, Cosimo Pazzi, from being invested as archbishop of Florence.⁴¹ In 1510

³⁷ Filippo's defense can be pieced together from the official report of the sentencing, from letters, and from Lorenzo Strozzi's account of the trial in his biography of his brother. See ASF, *Otto di Guardia e Balìa, Partiti e Deliberazioni*, 143, ff. 23–25; Filippo Strozzi to Antonfrancesco degli Albizzi, December 22, 1508, ASF, CS, 3d ser., 134, f. 51; and Strozzi, *Vita di Filippo Strozzi*, xx–xxii.

³⁸ According to Lorenzo, there was a normal increase in the amount of all fines paid to the *Otto di Guardia*; *Vita di Filippo Strozzi*, xxiii. For record of the payment, which finally amounted to 694 florins, in a seventeenth-century copy, see ASF, CS, 3d ser., 97, f. 56: "Filippo di Filippo Strozzi dee dare a dì 18 Gennaio 1508 F. 826. 14. 8 a giorno detto per valuta di F. 694 d'oro e y. 5 s. 8 ebbe conto portò lui detto disse per dare a Girolamo di Attaviato Geriti, Proveditore degli Otto per conto della condennazione fatoli dal presente ofitio di Otto per avere tolto per donna la figliuola di Piero de Medici."

³⁹ The *Signoria* voted to order the *Otto di Guardia* to summon Filippo back to Florence without risk of violating the terms of his confinement, and he was allowed to remain by vote of the *Signoria* in December 1509; Giovanni del Cast^{no} to Luigi Guicciardini, ASF, CS, 3d ser., 220, f. 99: "... vi aviso come da gli M.^{ci} S.^{ci} che hora seggono gl'è facto un partito con g fave che lui potesse stare tutto questo mese di dicembre 1509 in firenze sano o infermo et come allui piacesse: parsi sia volto alla stare infermo perché è inersia, enfiatogli la gola e'l viso et non pare stia troppo bene."

⁴⁰ For a discussion of the evidence to support this point, see my "Filippo Strozzi: Favor and Finance under the Medici in Florence and Rome during the Early Sixteenth Century" (Ph.D. dissertation, Cornell University, 1977), 135–38.

⁴¹ Francesco Guicciardini's marriage to the daughter of Alamanno Salviati, a friend turned opponent of Soderini, and the similar case of Alessandro Sachetti are examples of undesirable betrothals that Soderini was unable to prevent; see ASF, *Otto di Guardia e Balìa, Partiti e Deliberazioni*, 149, f. 155. On the election of Cosimo Pazzi as archbishop in 1508, see Cambi, *Istorie fiorentine*, 161; Jacopo Nardi, *Istorie della città di Firenze*, 2 vols. (Florence, 1856), 1: 32, 95, 196–202; Cerretani, *Istoria fiorentina*, f. 320; Nerli, *Commentari de' fatti civili occorsi dentro la città di Firenze*, 162; and Piero Parenti, *Istorie fiorentine*, BNF, MS II. IV., 171, f. 10v.

Prinzivalle della Stufa plotted to assassinate Soderini and reinstate the Medici, a plot that was aborted only at the last moment.⁴² Even though Soderini enjoyed a brief resurgence of power in the government as part of a backlash against the conspiracy, his situation continued to worsen. His support of the schismatic Council of Pisa—an affront to Julius II—pushed relations with the papacy beyond the point of reconciliation. Finally, in 1512 a group of disgruntled aristocrats led by Antonfrancesco degli Albizzi forced Soderini to flee, and they threw open the city gates to Cardinal de' Medici, who was waiting outside at the head of a papal army.

Scholars have emphasized the basic continuity of the power and influence of patrician families in Florentine government, whether during the Medici regime of the fifteenth century or the republican government after the family's exile.⁴³ Although the republic allowed much wider participation in government than had been the case under the Medici, the great oligarchic families wielded an influence disproportionate to their numbers during the republican period, and the amount of patrician support enjoyed by a particular regime constituted a vital clue to its stability and well-being. Soderini's election as *gonfaloniere a vita* in 1502 was itself the result of a brief coalition to strengthen the aristocratic hold on the government. Soderini's own political troubles began when he tried to introduce into the government "new men," men loyal to himself alone, thereby awakening the jealousy and then the opposition of many of his original supporters among the most powerful Florentine families. And the conspiracy of Prinzivalle della Stufa in 1510, historians have claimed, marked the turning point in this process of disaffection; it was also a critical juncture in Soderini's relations with the papacy and contributed to Pope Julius II's willingness to sponsor Cardinal de' Medici's return to Florence in 1512.⁴⁴ But the turmoil engendered by the Strozzi-Medici betrothal shows how far the disintegration of Soderini's political position both at home and with

⁴² Filippo was implicated in the plot because Prinzivalle, a childhood friend, tried unsuccessfully to enlist his cooperation in the ill-conceived plan; Bullard, "Filippo Strozzi: Favor and Finance under the Medici," 150–61. For contemporary accounts of the plot, see Cerretani, *Istoria fiorentina*, ff. 354–63; Strozzi, *Vita di Filippo Strozzi*, xxvi–xxix; Nerli, *Commentari de' fatti civili occorsi dentro la città di Firenze*, 167–68; Nardi, *Istorie della città di Firenze*, 2: 11–13; Cambi, *Istorie fiorentine*, 243–48; Parenti, *Istorie fiorentine*, ff. 45–47; and Luca Landucci, *Diario fiorentino dal 1450 al 1516*, ed. Iodoco del Badia (Florence, 1883), 304–05.

⁴³ For the most comprehensive treatment of the Medici government of the fifteenth century, see Nicolai Rubinstein, *The Government of Florence under the Medici* (Oxford, 1966). Also see L. F. Marks, "The Financial Oligarchy in Florence under Lorenzo," in E. F. Jacob, ed., *Italian Renaissance Studies* (London, 1960), 123–47. For the republican period, see Nicolai Rubinstein, "Politics and Constitution in Florence at the End of the Fifteenth Century," in Jacob, *Italian Renaissance Studies*, 148–83, and "I primi anni del Consiglio Maggiore di Firenze (1494–1499)," *Archivio storico italiano*, 112 (1954): 151–94; Felix Gilbert, *Machiavelli and Guicciardini: Politics and History in Sixteenth-Century Florence* (Princeton, 1965); and Sergio Bertelli, "Machiavelli e la politica estera fiorentina," in Myron P. Gilmore, ed., *Studies on Machiavelli* (Florence, 1972), 29–72, and "Pier Soderini 'Vexillifer Perpetuus Reipublicae Florentinae,'" in Anthony Molho and John A. Tedeschi, eds., *Renaissance Studies in Honor of Hans Baron* (Florence, 1971), 333–59. For the whole period leading up to the institution of the principate in Florence, also see Rudolf von Albertini, *Das florentinische Staatsbewusstsein im Übergang von der Republik zum Prinzipat* (Bern, 1955).

⁴⁴ Bertelli, "Machiavelli e la politica estera fiorentina," 49–50, 69, and "Pier Soderini," 333–38, 354–56. Sergio Bertelli's research furnishes a corrective to Roslyn Pesman Cooper's view of the "popular" character of the regime; see Cooper, "L'elezione di Piero Soderini a Gonfaloniere a vita: Note storiche," *Archivio storico italiano*, 125 (1967): 146–85.

the papacy had already progressed by 1508. The political issues surrounding Filippo's marriage, thus, turn the clock back on Soderini's political troubles to an earlier time than has generally been assumed. Not only did Soderini's pointed exchange with Julius II give clear testimony to the *gonfaloniere's* already deteriorating relations with the pope, but Soderini's failure to intimidate the growing numbers of aristocratic opponents supporting the marriage, to punish Filippo severely, or even to use the issue to influence the outcome of the elections pointed forcefully and publicly to the inevitability of his downfall.

AMONG THE ELITES of sixteenth-century Florentine society, family and politics were often two sides of the same coin. The Strozzi-Medici marriage, although it quickly exploded into a burning political issue, was still at heart a family affair. Soderini was not as upset that Clarice found herself a husband as he was enraged that her family blatantly attempted to use her to re-establish itself among the Florentine aristocracy. For her uncles and brother Clarice was above all a tool of family policy, a pawn to be risked for the greater good of securing their family's future in Florence. For the Medici family Clarice's marriage to Filippo was a decided victory and proclaimed, in conjunction with the election of Archbishop Pazzi, the growing influence of the Medici in Florence. The Strozzi-Medici alliance signaled their success in uniting with aristocratic families, even with those that had been their traditional enemies in the fifteenth century, such as the Pazzi and the Strozzi. For the Strozzi the significance of Filippo's act was not as direct. Unlike Clarice, Filippo was not part of any predetermined family strategy, but his marriage still aroused the grave concern of his kin. The problem of how to interpret the role of the Strozzi family in this affair leads to an area currently the subject of much historical debate.

Renewed interest in the family among social historians has sparked a re-examination of traditional views on family structure and intrafamilial relations in pre-industrial European society. At issue in Renaissance scholarship in broad terms is the relative influence exerted by the bonds of the small nuclear family as against those of the larger lineage, the so-called extended family.⁴⁶ Recently, historians have disagreed whether or not during the course of the Renaissance the nuclear family developed into the dominant social unit while the claims of lineage declined to relative unimportance. Jacob Burckhardt's belief in the rise of Renaissance individualism, when applied to the family, would seem to argue for the successive breakdown of lineage ties. For out of the disintegrating corporate structure of medieval society would emerge individuals more oriented toward their immediate households and less dependent upon larger familial associations. Ten years ago Richard Goldthwaite, in an impressive study of the private wealth of four Florentine families, saw

⁴⁶ See note 7, above.

confirmation of the break-up of the bonds of horizontal and vertical lineage in the existence of independent nuclei.⁴⁶ The very nature of his sources—primarily family account books—tended to highlight the diversity of economic interests among different households in the four families he studied and, thus, to demonstrate his evolutionary view of the family. Additional evidence of a different nature—political records and, particularly, private family papers and correspondence⁴⁷—has, however, complicated the issue: the family was a much more complex social entity than previously thought, and the existence of strong independent households and strong lineage ties were not necessarily mutually exclusive. No definitive resolution of the issue has yet emerged. If both types of family ties continued to have relevance in Florentine society, the problem of assessing their relative weights remains. Here the case of the Strozzi is highly instructive, especially since theirs was one of the largest of Florence's patrician families, boasting twenty-eight households by the mid-fourteenth century⁴⁸ and consequently highly diverse in economic circumstance and political fortune.

Of what significance, then, were Filippo's wider Strozzi ties at the time of his marriage? How did his Strozzi kin react to the news of his betrothal to a Medici? What form did their reaction take, and what can it reveal about the strength of their bonds of kinship? On the surface Filippo's plan to wed Clarice de' Medici constituted a totally independent course of action, bucking seventy-five years of traditional family loyalties. He and his mother had conducted the negotiations clandestinely, keeping the matter hidden from other Strozzi, even from Filippo's half-brother Alfonso, who shared the family palace. But neither the members of the wider Strozzi kin group nor even Filippo himself ever conceived his daring course of action as defying the needs and concerns of the rest of the Strozzi family.

Just how vibrant and alive that family consciousness was came out in the line of defense Filippo presented at his trial, when he evoked for the court the strong sense of Strozzi history and tradition and placed his Strozzi loyalties

⁴⁶ Goldthwaite, *Private Wealth in Renaissance Florence*, 251–75.

⁴⁷ For the use of some of these types of sources, see Rubinstein, *The Government of Renaissance Florence under the Medici*; and Dale Kent, "The Florentine 'Reggimento' in the Fifteenth Century," *Renaissance Quarterly*, 28 (1975): 575–638. The thesis that bonds of lineage determined the structure of Florentine patrician society notably underlies Martines's *The Social World of the Florentine Humanists*. Francis William Kent's contribution in *Household and Lineage in Renaissance Florence* has been to view a century in the history of the Capponi, Ginori, and Rucellai families in terms of their bonds of kinship, stressing the co-existence of different kinds of familial relationships throughout the fifteenth century. Kent has rightly emphasized that the strongest familial ties were patrilinear, but he has perhaps overstressed that observation to the point of obscuring the family bonds that wives brought to their husbands. In the case of the Strozzi, Lucretia Rucellai's marriage to Lorenzo brought her father and brothers into close involvement with Strozzi affairs. And Filippo's marriage to Clarice clearly demonstrates that the powerful political connections she brought to the Strozzi were the most instrumental factors in shaping the future of that family. Nor is there any evidence whatsoever that after her marriage Clarice ever left the Medici orbit or that her identity as a Medici was in any way diminished by her position as the wife of a Strozzi.

⁴⁸ Goldthwaite, *Private Wealth in Renaissance Florence*, 33. On the origins and early history of the Strozzi family, see P. J. Jones, "Florentine Families and Florentine Diaries in the Fourteenth Century," *Papers of the British School at Rome*, 24 (new ser., 11) (1956): 183–205. The Strozzi became widely dispersed as a result of successive decrees of exile. Prominent branches grew up in Ferrara and Mantua, but Strozzi relatives also settled in the other cities mentioned by Filippo in his defense before the *Otto di Guardia*.

above all others. That line of argument, appropriate for the nature of the trial, might lead to the conclusion that it was devised especially for the occasion. Yet Filippo's most intimate correspondence with his brother Lorenzo, written in a moment of great tension and indecision about returning to Florence to answer the summons of the *Signoria*, reveals that both men actively considered more than just their personal fortunes. "What can I or should I do?" Filippo asked. "If I sever my ties with the Medici, which seems impossible, aside from the penalty to pay, we would bring down their wholehearted hostility and I would be dishonored. If I go through with it, however, you say that I and all of us will be ruined, and you depict an inferno so black that it scares me. . . . I am not writing to you like this for my own sake, and my opinion remains unchanged, but rather regarding the danger to you, because I do not want you to lose on my account anything that I cannot restore to you."⁴⁹

The initial impact on the family of Filippo's news leaves no doubt whatsoever about the vitality of the sense of shared identity and loyalty prevalent even among far-flung relatives and associates. Their anguished cries echoed from as far away as Venice and Ferrara: How could Filippo betray the memory of his father and grandfather, who had suffered so much in exile at the hands of Clarice's ancestors? "We have read at length what you have written about Filippo's betrothal to the daughter of Piero de' Medici, and we are absolutely certain that you are terribly upset over it, just as you have every right to be," wrote Leonardo and Niccolò, Strozzi business associates in Venice. "Out of our love for you and for him we also have been and still are distressed, because it seems to us that such a course of action is hardly appropriate in the Strozzi family." Giovambattista Ciei likewise lamented, "What an odious matter and one so harmful to everyone in the family," particularly to Alfonso because of what his father and all of the other Strozzi suffered at the hands of the Medici family.⁵⁰ Ciei, who was in Florence, promised to keep Giovanni Strozzi in Ferrara informed daily of any developments in the case, and Giovanni was to advise other Strozzi in Ferrara and contact those in Mantua, for they were all "terribly worried and upset." Giovanni's nephew Marco Strozzi wrote his uncle about the *parentado* from Rome, where every detail was known.⁵¹ Such anguish expressed in the letters

⁴⁹ Filippo to Lorenzo Strozzi, December 9, 1508, ASF, CS, 3d ser., 134, f. 50: "Ora che posso o debbo fare? Se mene discosto, il che non so in che modo mi sia per riuscire, oltre alla pena bisognerà pagare, ci resta una nimicitia cordiale, et io ci resto vituperato. Se seguito, tu mi di che io e tutti sian' rovinati, e dipignimi uno inferno sì scuro che io mi smarrisco. . . . Io non mi muovo a scriverti per conto mio in questa forma e sono dell'animo sono sempre stato, ma e pericoli tua, che non vorrei a mia ragione perdessi quello che poi restituire non ti potessi."

⁵⁰ Leonardo and Niccolò to Lorenzo Strozzi (Venice), December 30, 1508, ASF, CS, 3d ser., 180, f. 95: "Abbiamo inteso alungo quanto scrivete sopra il parentado fatto dal vostro Filippo in torre per donna la figlia di Piero de' Medici di che siamo certisimi avete auto gran dispiacer' per più conti che veramente avete ragione, e noi al sì per amor vostro e suo n'abbiamo auto e abiam', che non ci pare tal cosa sia punto al proposito della casa vostra. . . ." Giovambattista Ciei to Giovanni Strozzi, December 2, 1508, ASF, CS, 3d ser., 134, f. 49: "Che materia odioxa da nuocere a tutti di chasa," and December 16, 1508, ASF, CS, 3d ser., 134, f. 52: "[A Alfonso] questa cosa li dispiace troppo, richordandosi quanto male anno ricevuto dalla chasa de' Medici loro padre, Messer Palla e degli altri."

⁵¹ Giovambattista Ciei to Giovanni Strozzi, December 16, 1508, ASF, CS, 3d ser., 134, f. 52: "Raghuagliatene Messer Carlo e li altri consorti, simile ne scriverete a Mantova, e io v'aviserò giornalmente di

and innermost reflections of even distant cousins shows how quickly the Strozzi's indignation could surface over what they perceived to be an affront to family honor.

This striking reaction to the news by the rest of the Strozzi indicates to what degree Selvaggia and Filippo had indeed set themselves against the prevailing political current in the family, but it also shows that the greater Strozzi kin group, far from being just a number of loosely connected households that shared the same cognomen, had intimately concerned itself with Filippo's decision and was capable of concerted action on a matter of common family interest. Summoned by Alfonso and Matteo Strozzi, leading members of the family met together on December 3 to decide a course of action. Two sentiments dominated the discussion: genuine distress and indignation that Filippo had allied himself with the Medici—of all families—and fear that he had placed not only himself but the whole Strozzi family in danger of grave political reprisal from the Soderini government. The latter anxiety, in particular, determined the Strozzi's subsequent public actions.⁵² They sought the best way to annul Filippo's engagement to the Medici; by openly registering their discontent over his betrothal, they hoped to clear themselves of suspicion with Soderini. Thus, they went as a group before the *Signoria* to explain their innocence and ignorance of Filippo's deed and promised to do all they could to stop the marriage.⁵³ Next, the Strozzi dispatched one of their number, Benedetto di Giovanni Strozzi, as a messenger to Cardinal de' Medici in Rome with a petition signed by all of the male members of the family, begging him to nullify Filippo's contract or else suffer the hatred of the Strozzi. Benedetto continued on to Naples, bringing letters admonishing Filippo to change his mind.⁵⁴ Alfonso Strozzi even offered to pay the two-thousand-ducat penalty to the Medici if Filippo would break the contract.⁵⁵

Strozzi antennae were sensitive to political change, and, as it became

quanto succederà che per questo tutti siamo forte ochupati e malchontenti." Marco Strozzi to his uncle Giovanni Strozzi, January 5, 1509, ASF, CS, 3d ser., 134, f. 61: "... qui sene sa il tutto di quanto segue hora per hora." For other family letters, see ASF, CS, 3d ser., 134, ff. 53 (Giovambattista Ciei to Giovanni Strozzi, January 11, 1509), 54 (Giovambattista Ciei to Giovanni Strozzi, January 19, 1509), 55 (Marco Strozzi to Giovanni Strozzi, February 3, 1509). Corroborating evidence that demonstrates the Strozzi sense of tradition comes from the period twenty years earlier, during the building of the Strozzi Palace; see F. William Kent, "'Più superba de quella de Lorenzo': Courtly and Family Interest in the Building of Filippo Strozzi's Palace," *Renaissance Quarterly*, 30 (1977): 311–23.

⁵² Giovambattista Ciei to Giovanni Strozzi, December 2, 1508, ASF, CS, 3d ser., 134, f. 49: "E domani raghuneranno tutti quelli della chasa per provvedere in tutti que' modi si possa di farlo stornare in dietro, quanto che uno sarà la rovina di Filippo et di chi ne sia stato chauxa, che questo regimento sia per farne dimostrazione e grande."

⁵³ Guicciardini, *Storie fiorentine*, 327–28.

⁵⁴ Giovambattista Ciei to Giovanni Strozzi, December 16, 1508, ASF, CS, 3d ser., 134, f. 52: "Di poi tutta la chasa si raghunò e mandoseli una stafetta a Nnapoli che non si partissi e non tirassi più avanti la cosa sino intendessi altro, e apresso chol consenso della Signoria si mandò Benedetto di Giovanni Strozzi con una soscrizione di tutta la chasa a Roma al Chardinale de' Medici per vedere se per amore e per grazia potesse annullare el merchato, rimostrando a sua Rev.^{ma} S. se faceva parentado con Filippo, faceva nimicizia con tutti di chasa col quale non a posuto fare ne avere che parole e se n'è andato a Nnapoli a fare intendere a Filippo quanto sia suto malchonsigliato e rimuoverllo, potendo, da tale opinione; altrimenti ne seguirà la ruina sua e di qualchun altro."

⁵⁵ Giovambattista Ciei to Giovanni Strozzi, December 16, 1508, ASF, CS, 3d ser., 134, f. 52: "[Filippo] s'è obligato per iscripta sotto pena di 2000 ducati, e quali Alfonso vuole paghere lui. . . ."

increasingly apparent that Soderini would have difficulty mustering enough support to deal harshly with Filippo, the family members began to soften their stance. Shortly after the *Signoria* issued its summons in December, the family called those Strozzi males of reputation and standing who had reached the age of majority to a second meeting.⁵⁶ We will never know exactly what happened, what petitions Filippo presented through his letters, and what specific arguments were exchanged in the meeting. But, in the name of family honor, those present decided to drop their opposition to Filippo's pending marriage and to stand behind him as he faced prosecution by the Soderini government. After consultation with Cardinal de' Medici in Rome, Filippo traveled to the Florentine border, where his first cousin and close friend, Matteo Strozzi, and a respected family lawyer, Antonio di Vanni Strozzi, met with him in secret to examine all angles of the case and prepare his defense.⁵⁷

In the days and weeks that followed, family and friends lobbied members of the government to guarantee Filippo's safety and fair treatment in the legal phase of his trial.⁵⁸ The number of Filippo's friends and supporters kept growing; the opposition to Soderini finally included not only the Strozzi and the Medici and their in-laws and clients but other aristocrats and their in-laws and clients as well. Meanwhile, the *gonfaloniere's* own block of support, at least among the patriciate, was rapidly melting away. With such defenders, it is not surprising that Filippo was able to avoid the punishment of exile and that he received a light sentence from the *Otto di Guardia*. This sort of family solidarity is exactly what the general discussion of family behavior and familial bonds in Florentine society has led us to expect. That Filippo's family stood behind him is less interesting than the manner in which it did so—that the Strozzi held family councils, went before the government as a family, and even reversed their original position on the marriage.

THE OUTCOME OF THIS WHOLE AFFAIR, moreover, had a lasting impact on Filippo and his family. In the years that followed, they came to regard the marriage as basic to their calculations and plans. In 1512, for example, shortly after the Medici had taken control of Florence by force, a distant cousin who felt disappointed that the new Medici government did not include more Strozzi burst out bitterly to Filippo, "Since the Medici have shown us that they do not consider our marriage alliance with them to be of any account, Filippo, if I were you, I would send that wife of yours, Clarice, packing back home to them."⁵⁹ Filippo certainly had no intention of sending his wife away,

⁵⁶ Strozzi, *Vita di Filippo Strozzi*, xvii–xviii: "Ristrinsonsi, dopo tali lettere, insieme i fratelli, e tutti quelli degli Strozzi che di maggiore età e reputazione erano; ed esaminata la giusta petizione di Filippo, e quello che all'onore loro conveniva, risolverono non mancargli di quegli aiuti e favori che onestamente per loro dare se gli potessino."

⁵⁷ Strozzi, *Vita di Filippo Strozzi*, xviii–xix.

⁵⁸ *Ibid.*, xviii.

⁵⁹ For Lorenzo Strozzi's report of the incident, see *ibid.*, xxxiii: "Poiché i tuoi Medici con questa ballia ci hanno mostro non tenere conto alcuno del nostro parentado; se a me stesso, io ne rimanderei la Clarice tua moglie a casa loro." Baroncello Baroncelli, long-time trusted friend and employee, wrote to Lorenzo

nor did the Strozzi lineage, as the frustrated cousin would have us believe, fail to benefit from its Medici connections. Before his marriage, Filippo had been but a fatherless son of one branch of the Strozzi family; but, after he married Clarice de' Medici, he became the recognized head of his entire lineage. When Clarice's brother controlled the Medici government in Florence after 1512, Filippo was the number-two man in the city. In Rome, after Clarice's uncle was elected Leo X, Filippo and his brothers were able to negotiate for lucrative positions at the *curia* with the help of her mother. Through his connections at the Medici court Filippo amassed a fortune and earned his reputation as the wealthiest banker in all Christendom after Jacob Fugger.⁶⁰ And Filippo constantly promoted the political fortunes of other Strozzi in Florence and brought them back into the mainstream of Florentine political life. He recommended family members for various magistracies and interceded with the Medici on behalf of his brother Lorenzo to advance his candidacy for the highest executive position in the city. A survey of Florentine election records very clearly shows that a large number of Strozzi relatives were favored with public offices after 1512—a complete reversal of their political circumstances in the fifteenth century under the previous Medici government, when the family had opposed the regime and had been completely excluded from office.⁶¹ All of these changes in the sixteenth century, first the rise of Filippo's political and economic stature within his own family and within Florentine society and then the reversal of the political fortunes of the Strozzi lineage in Florence, can be attributed to Filippo's close association with the Medici and can be traced directly back to that singular event in 1508, his marriage to Clarice de' Medici.

The Strozzi-Medici marriage was in many ways exceptional in Florence's history. There was only the one daughter of Piero de' Medici and granddaughter of Lorenzo the Magnificent, whose marriage in Florence in 1508 represented a major threat to the security of the state. No other marriage in the history of the city was so politically charged that it caused such public furor, aroused such bitter feelings among factions in the city, and precipitated a major governmental crisis. But the exceptional features of the marriage should not obscure what it more generally elucidates about family behavior in Renaissance Florence. In one basic sense it is fortunate that the marriage was such a *cause célèbre*: By virtue of its notoriety, the match was extensively documented and recorded by contemporaries. Without those valuable records, there would not be such clear proof of the solidarity of family ties

Strozzi on September 20, 1512; see ASF, CS, 3d ser., 134, f. 76: "... in vero mi pare strano non ce n'essere in casa, ma forse per aventura si potrebbe dire se ne son cagion' chol'essersi un pocho portati freddi overo lenti a lor' favori."

⁶⁰ Rabelais made this comparison; see his *Les lettres écrites pendant son voyage d'Italie* (Brussels, 1710), 6. For an extensive treatment of Filippo's rise to favor and financial success, see my *Filippo Strozzi and the Medici: Favor and Finance in Sixteenth-Century Florence and Rome* (forthcoming).

⁶¹ Bullard, "Filippo Strozzi: Favor and Finance under the Medici," 204–08. For the election records, see ASF, Tratte 52, 84, 177, 342. Filippo's correspondence contains many requests for favors or for his intercession with the Medici from friends and dependents, which only increased in number in later years as his position with the Medici grew more powerful.

among the patrician families of Renaissance Florence. The seriousness with which the Strozzi in particular and the Florentines in general treated this specific marriage and the extremes to which they went to defeat or defend it are sure indications of the deep political as well as social repercussions of marriage and family ties in their culture.

Review Article

Pluralist Politics in British India: The Cambridge Cluster of Historians of Modern India

HOWARD SPODEK

- BAKER, CHRISTOPHER JOHN. *The Politics of South India, 1920–1937*. Cambridge, 1976.
- BAYLY, C. A. *The Local Roots of Indian Politics: Allahabad, 1880–1920*. Oxford, 1975.
- BROWN, JUDITH M. *Gandhi and Civil Disobedience: The Mahatma in Indian Politics, 1928–34*. Cambridge, 1977.
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- JOHNSON, GORDON. *Provincial Politics and Indian Nationalism: Bombay and the Indian National Congress, 1880 to 1915*. Cambridge, 1973.
- ROBINSON, FRANCIS. *Separatism among Indian Muslims: The Politics of the United Provinces' Muslims, 1860–1923*. Cambridge, 1974.
- SEAL, ANIL. *The Emergence of Indian Nationalism: Competition and Collaboration in the Later Nineteenth Century*. Cambridge, 1968.
- TOMLINSON, B. R. *The Indian National Congress and the Raj, 1929–1942: The Penultimate Phase*. London, 1976.
- WASHBROOK, D. A. *The Emergence of Provincial Politics: The Madras Presidency, 1870–1920*. Cambridge, 1976.

THE HISTORIAN OF MODERN INDIA confronts vital and exciting issues: cultural continuities of extraordinary duration and their transformation; imperialism and nationalism; regional integration and separatism; and poverty and

I would like to thank many people for their useful suggestions on earlier drafts of this paper, including some of the scholars whose work is reviewed here: Christopher Bayly, Judith Brown, Gordon Johnson, B. R. Tomlinson, and David Washbrook. Others include Bernard Cohn, Robert Frykenberg, Douglas Haynes,

planned economic growth. And nineteenth- and twentieth-century India provides the opportunity to compare theories of modernization against the realities of one of the oldest of cultures. Further, just as India's experience under and after British rule frequently served as a benchmark for imperial administration and for the first stages of independence in other countries, so, too, the historical study of that experience has been closely related to similar study of imperial rule and its successors elsewhere. Developments in the historiography of modern India, therefore, have significance for historians working in widely diverse geographical areas.

These issues and opportunities in modern Indian history challenge the methods of the social scientist and the empathy of the humanist; they demand skills of attention to both detail and grand sweep. Yet relatively few historians have specialized in modern India, and few students are now choosing to study the subject, suggesting that the field may shrink still more. Because the field is small, the center of modern Indian history at Cambridge University has exercised extraordinary influence over it. The publications emanating in the past decade from that center include the ten books examined here, nine of which have appeared in the last seven years. Unlike any other major center of modern Indian history,¹ the Cambridge group has kept a single issue near the forefront: the penetration of nationalist organization to subnational levels.

The time has come (1) to analyze the basic perspective of the school, (2) to examine each of its major works, (3) to assess them as a whole for their contributions—and their drawbacks—in establishing a new framework for understanding the nationalist period in modern Indian history, and (4) to propose an alternative framework for that understanding, based on the Cambridge historians' insights yet diverging sharply from them in interpretation.

THE INTELLECTUAL IMPETUS for the examination of symbiotic interaction between local groups and anticolonial nationalist movements began at Oxford with the work of John Gallagher and Ronald Robinson on imperialism in Africa. In a highly influential article written a quarter of a century ago, Gallagher and Robinson began to develop their argument that imperial rule spread in Africa in large part through the collaboration of Africans, who found personal and group advantage in working with the colonial powers.²

Richard Lambert, Susan Lewandowski, John B. Harrison, Tom Metcalf, and B. N. Pandey. I am also indebted to Barbara Metcalf, who was kind enough to turn over a small seminar session at the University of Pennsylvania to a discussion of this paper. Finally, I would like to thank the tolerant and helpful members of a history department seminar at Temple University, where I presented the earliest draft. Needless to say, only I can be held responsible for the final result. I prepared this essay while carrying out research projects on the historiography of Indian urbanization and on Sardar Vallabhbhai Patel; I am grateful to Temple University's Faculty Senate and to the American Philosophical Society respectively for summer grants in 1977 and 1978 that supported these projects.

¹ Other major centers include the University of New South Wales and the Australian National University in Australia; the Universities of Chicago, California at Berkeley, Pennsylvania, Washington, and Wisconsin and Duke University in the United States; the School of Oriental and African Studies, University of London, in England; and Delhi University and Jawaharlal Nehru University, New Delhi, in India.

² Gallagher and Robinson, "The Imperialism of Free Trade," *Economic History Review*, 2d ser., 6 (1953): 1-15.

Later, they carried the argument forward in time toward the obverse process, the development of anti-imperial nationalism engineered by colonized groups who chose to collaborate no longer. "The work of Robinson and Gallagher has made it clear," according to William Roger Louis, "that indigenous collaboration and recalcitrance helped to shape European penetration into some non-Western societies, that collaboration guided the direction and form of colonial control, and, by extension, that the inversion of collaboration into non-cooperation determined in large part the process of decolonization." And, Louis continued, "their insights can be applied to various regions and different eras."³

Gallagher's principal interest lay in Africa, but at Oxford he supervised the dissertation of Anil Seal (and, several years later, that of Christopher Bayly as well). Seal applied Gallagher's concepts to India; and Seal's dissertation, which forms the basis of *The Emergence of Indian Nationalism*, describes a process of collaboration between the imperialist rulers and specific educated elites in India, in which the elites decided that their best interests required at least some degree of opposition to foreign rule. Once Seal became a Fellow of Trinity College and a lecturer in history, his students in turn began to explore the patterns of collaboration with, and opposition to, imperial rule at sub-national levels. This core of students clustered around Seal at Cambridge produced all of the remaining books considered here except Bayly's *The Local Roots of Indian Politics*. Nine of the volumes are monographic, while one—*Locality, Province, and Nation: Essays on Indian Politics, 1870 to 1940*—is a collection of articles that first appeared in 1973 as a full issue of Cambridge University Press's quarterly, *Modern Asian Studies*.

Seal's introduction to this volume, "Imperialism and Nationalism in India," summarized the central outlines of the group's inquiries. It stressed "two main arguments": First, "Indian politics were an interconnected system working at several levels; and government had much to do with the linking of those levels." Second, "imperialism built a system which interlocked its rule in locality, province, and nation; nationalism emerged as a matching structure of politics. Indian politics have to be studied at each and every level; none of them can be a complete field of study on its own."⁴

The first moving engine in the emergence of a modern political nation in India, then, was British imperialism. The British imposed an administrative and political matrix in which the Indian nationalist organization took shape. As the British expanded from their first bases of territorial rule in Madras, Calcutta, and Bombay in the late eighteenth and early nineteenth centuries, they established a system that successively and successfully incorporated all of upper India, South India, and western India. They actively encouraged Indian participation in the governing framework in the lower ranks of the civil

³ Louis, ed., Introduction to *Imperialism: The Robinson and Gallagher Controversy* (New York, 1976), 37. In this reader, Louis reprinted "The Imperialism of Free Trade" in full, excerpts from other crucial studies by Gallagher and Robinson, and a number of scholarly responses to their work.

⁴ Seal, "Imperialism and Nationalism in India," in Gallagher, Johnson, and Seal, eds., *Locality, Province, and Nation*, 24, 27.

service. But, as Seal has pointed out in *The Emergence of Indian Nationalism*, only a few of the viceroys after the revolt in 1858, notably George, Lord Ripon (1880–84), began to include Indians in the formal decision-making process and to encourage their participation in elected government at the local level. Like Edward Lord Lytton (1876–80), most of the viceroys did not much like educated Indians and employed them only to save money (Indians drew lower salaries than British employees). As Indians began to participate in electoral politics and to hold policy-making positions within the administration, however, they increasingly sought to turn the raj to their own uses.

In the twentieth century, power became more accessible to Indians because the external pressures of two world wars and an international economic depression forced the British to economize. The imperial government followed a policy of devolution of power in India. In two sets of political reforms, in 1919 and in 1935, the British granted increasing power to provincial administrations governed by Indian officials, who were chosen by continually expanding electorates, but from its New Delhi enclave the imperial government maintained control of defense, foreign affairs, and the treasury. And the British reserved the right, through their governors in the provinces, to set aside provincial legislation that they deemed inappropriate. Devolution, Seal has claimed, relieved pressure from the British administration: it charged Indians with some of the unpleasant responsibilities of levying taxes and of finding inexpensive personnel to maintain the bureaucracy, but it did not sacrifice real power.

In evaluating the motives of Indians entering into the expanded opportunities left by the retreating British, Seal has revised the significance previously placed on the politics of nationalism. The principal concern of most Indians in the national movement was not, as many historians have argued, the achievement of independence, nor was it even an increasing share of power at the national level. Rather, Indians at the local level sought British support for their own interests at that local level. Frequently, rival factions in the village, district, or province sought connections with the British to strengthen their own positions. In response, the faction that could not win the British connection went into opposition. Immediate grievances were frequently mundane, even petty; they reflected local problems and contests for power rather than opposition to British rule in India *per se*.

As the British administrative net spread across the country, parties with local grievances could more easily seek allies both at these higher levels of power and also at parallel levels of power in other localities. Just as the British had their network and linkages for governing, so Indians began to develop a parallel network and linkages to advance their own concerns. The search for allies, vertically and horizontally, led to the formation of national political organizations. Public political figures attempting to build national organizations engaged in exalted rhetoric and eventually came to see the expulsion of the British as the key to India's overall development, but much of the popular involvement in nationalist political activity reflected local issues. Seal's re-

assessment of nationalism calls for a two-level study: first, a study of local issues, factions, and organizations; and, second, the study of the linkages between these local elements and those outside the locality, either at the higher levels of province and nation or at parallel levels in other provinces. Although Seal has not made any reference to the work of Sir Lewis Namier, some of Seal's concerns with respect to nineteenth- and twentieth-century India recall those of Namier with regard to eighteenth-century England: a concentration on personal interests, an examination of the mesh between local interests and national organizations, and a sharp discounting of the significance of ideological rhetoric.

Much to his credit, Seal himself has pointed out the degree to which this assessment of the nationalist structure, published in 1973, requires a re-evaluation of his own earlier book, *The Emergence of Indian Nationalism*. Published in 1968, that work was to be the first of a projected five-volume series on politics in modern South Asia, edited by Seal and John Gallagher, but none of the subsequent volumes has yet appeared. *The Emergence of Indian Nationalism* argues that nationalism in India was a product of the newly Western-educated elite. This elite—in touch with new ideas, fluent in English, and desirous of expanding job opportunities in government—was the first to join together in a nationwide network for political action. Already employed frequently in the British administrative network, it had the talents and training to establish national linkages and to promote new organs of propaganda. Seal's more recent views, however, project a much broadened political spectrum, in which the Western-educated groups are one example of much more widespread political associations. Cash-cropping farmers, religious leaders, businessmen, and many others were sooner or later drawn into political activity by the expanding activities of the British government. These groups may have had less experience in public expression, and their limited competence in a link language may have slowed them, but they entered politics on their own. They did not need the educated elites to organize them or to make them aware of their needs; they could represent themselves. They sought British help in local disputes over land, religion, and favors in trade, just as the educated did in the search for government jobs.

Propaganda elicited but little participation in politics; concrete local issues stirred the necessary passions. Seal, therefore, has evaluated the claims of political rhetoric in terms of its organizing power rather than its literal promises. In "catching" Indian politicians "out at their tricks and dwelling on their inconsistencies, historians are simply playing a game at which administrators used to excel. But to demonstrate that Indian politicians were not always what they claimed to be is not to describe what they were." Seal has recognized that "functional analysis" of propaganda is no easy task, but he has suggested that the Cambridge group's approach may make easy moral judgments "more difficult" and hard functional evaluations "less arduous."⁵

⁵ *Ibid.*, 25.

Healthy skepticism over ideological statements, Seal has argued, should lead to a fuller contextual analysis of their significance. Seal exhibited this level of contextual understanding when he assessed, on the British side, the changing viceregal policies from 1876 to 1888. He elaborated the reasoning behind Lytton's favoritism toward the landed aristocratic classes, Ripon's reversal in favor of the educated, Westernized, urban groups, and Dufferin's attempt to find a middle course. He examined the viceroys' personalities, their political party affiliations, and the forces and events with which they had to cope. Here skepticism engendered careful research. Unfortunately, Seal's views of Indian positions have been less tolerant, partly because with the Indians he has ploughed new ground.

Seal has asked new questions of the nationalists, leaving behind the conventional questions of how India won independence, to explore how the system of pluralist politics, which has marked independent India since 1947, originally took root under the raj. His implied *terminus ad quem* is thus not 1947 but rather the post-1947 epoch of democratic, multi-party politics.

The techniques and methods Seal has employed in his exploration have included political sociology, quantification, and a careful analysis of actual behavior as against public propaganda. His search for the constituent elements of the nationalist movement led him in *The Emergence of Indian Nationalism* to quantify information on the recently educated classes—by, for example, province, caste, occupation, religion, membership in organizations, and parental occupation and income—and to trace changes in these factors over some time. His later article, "Imperialism and Nationalism in India," summarized the research of his students, who have extended this analysis to additional political constituencies. Yet Seal's methodological concerns for disaggregating data, for attention to specific detail, and for understanding the local components of national events have not dulled his presentation. Writing well and movingly, he has maintained the sweep of major cultural and political transformations even when analyzing specific details. Seal is both a social scientist who writes well and a humanist who values specificity and methodological innovation. In revising his earlier views, he has obviously been open to the new findings that his own students have brought back from their field research. He has shown intellectual growth and encouraged others to grow as well. The Cambridge cluster has benefited from these internal reverberations.

SIX OF ANIL SEAL'S STUDENTS AT CAMBRIDGE and John Gallagher and one of Gallagher's students from Oxford have produced all of the remaining nine books reviewed here. The studies fall into four perspectives—(1) local, (2) provincial, (3) Muslim, and (4) national.

Christopher A. Bayly has written the only studies of a single locality. His preliminary article, "Patrons and Politics in Northern India," and his subsequent book, *The Local Roots of Indian Politics: Allahabad, 1880-1920*, provide a

Careful analysis of that city-region and focus most closely on its traditional elites, values, and customs. Bayly began his work as a student of Gallagher at Oxford and claims the most independence from the Cambridge cluster. Only after carefully analyzing local conditions did Bayly turn to the Cambridge group's concern for the linkage of the region to the national matrix. In Allahabad, the power of the traditional nobles, the *rais*, lay predominantly in their control of capital and credit. The *rais*, however, downplayed the importance of wealth, seeing it only a means to a more primary goal, the enhancement of Hinduism. The *rais* dispensed their wealth in charity for temple-building and religious schools and organizations.

During the pre-imperial years, in the absence of any official municipal government, these *rais* also functioned as the effective arbiters of the economic, religious, and social life of the city. When the British introduced municipal governmental structures after 1884, the *rais* entered formal politics. Economically, they wanted a voice in municipal taxation and expenditure; religiously, they wished to ensure that the great annual Hindu fairs of the region had proper supervision and that Muslim demands for burial grounds, cow slaughter, and special treatment for mosques did not impinge on their own Hindu interests. The fusion of these economic and political concerns with religious interests permeated northern India. By focusing on the projection of traditional values into modern politics, Bayly has highlighted one of the richest explanatory potentials of Seal's model: the capacity of the emerging political system to absorb many local, "nonpolitical" issues. And, in contrast with others at Cambridge, Bayly has not tried to fit these values into the category of self-interest.

The five authors who have dealt with politics on the provincial level have had less contact with local cultures and have, instead, ranged more widely across geographical borders. John Gallagher, Gordon Johnson, and Richard Gordon have studied the contests for power between provincial and national leaderships. Although David A. Washbrook and Christopher A. Baker have looked at localities, they have emphasized the external relations of these localities as they grouped together into subdivisions of provinces, vied for attention in provincial capitals, and interacted with national leadership. More centrally, these historians have examined the economic and political relationships within the provinces, and, together, the five cover all three presidencies of India—Bengal, Bombay, and Madras—across the period from 1870 to 1937. The major issues that have emerged from their studies are (1) the federal nature of the Congress system as evinced by the struggles between the central and provincial leaderships, (2) the self-seeking of many leading politicians, and (3) the importance of rural economic interests in political organizations.

Three major analyses of Bengal have stressed the conflicts between provincial politicians and the central leadership of the Indian National Congress. In each major decision, the Bengalis lost out. Gordon Johnson's "Partition, Agitation, and Congress: Bengal, 1904–1908," and John Gallagher's "Congress in Decline: Bengal, 1930–1939," have described two conflicts, a genera-

tion apart, between the interests of Bengali leaders and the concerns of the Congress leadership. In each case the center won out and, in the process, weakened the provincial leadership. Both Gallagher and Johnson have described the pluralist nature of the local political system and the federal relationships that began to develop between the provincial and national levels. Both have also stressed the self-seeking among the leaders: Hindus of West Bengal, Johnson has argued, opposed partition in 1905 out of fear of the loss of job opportunities in predominantly Muslim East Bengal. Similarly, Gallagher has seen the conflict between the Calcutta-based middle classes and the rural Bengali agriculturalists as a clash of small group interests rather than a conflict involving national issues.⁶ Richard Gordon's "Non-Cooperation and Council Entry, 1919 to 1920," charts an additional provincial contest that surfaced in 1920 as Congress debated how far to extend noncooperation. In describing Gandhi's success in gaining a Congress boycott of the elections, Gordon has demonstrated the concern of provincial congress leaders for the political capacities of their own particular provinces, sometimes to the exclusion of national interests.⁷

Across the subcontinent from Bengal, Gordon Johnson's *Provincial Politics and Indian Nationalism: Bombay and the Indian National Congress, 1880-1915*, also examines provincial-national linkages and treats them primarily as personality conflicts. Within Bombay a small group of Chitpavan Brahmins, divided between moderates and extremists, controlled the congress. Each faction sought allies in the other provinces and among national leaders. The national leaders, fearing polarization, sought to moderate the provincial conflict, isolate the extremists, and reconcile the majority within the fold of the national party. They sought a federal solution encouraging lively activity in these provinces, fostering the general process of linking locality to province and nation, and quarantining extremists.

David Washbrook and Christopher Baker have studied the third major presidency, Madras. Together their works cover the period from 1870 to 1937, during which time two great movements dominated Madras politics: the rise of the nationalist movement and the emergence of communal politics. Washbrook and Baker have examined the linkages that underlay these political movements from the apex perspective of British policies of withdrawal, through the responses within the provincial system, on down to local involvement—or lack of it.

In "Country Politics: Madras, 1880 to 1930,"⁸ and in his book, *The Emergence of Provincial Politics: The Madras Presidency, 1870-1920*, Washbrook has looked at the rise of nationalism first in terms of local agricultural ecology: He contrasted Madras's wet, irrigated areas of substantial market farming, broad-

⁶ Johnson, "Partition, Agitation, and Congress: Bengal, 1904-1908," and Gallagher, "Congress in Decline: Bengal, 1930-1939," in Gallagher, Johnson, and Seal, *Locality, Province, and Nation*, 213-68, 269-325.

⁷ Gordon, "Non-Cooperation and Council Entry, 1919 to 1920," in Gallagher, Johnson, and Seal, *Locality, Province, and Nation*, 123-53.

⁸ Washbrook, "Country Politics: Madras, 1880-1930," in Gallagher, Johnson, and Seal, *Locality, Province, and Nation*, 155-211.

based ownership of land, and comparatively wide political participation with dry, unirrigated areas with their small numbers of landowners, subsistence farming, minimal political participation, and suppression of the peasants at the hands of the local landowners. Nationalism, Washbrook has argued, quickly took root in the wet areas but not in the dry, suggesting that “the best way in which we can begin to analyze the socio-political structure of the presidency is by examining the bases of economic organization.”⁹ On the second great issue, the rise of communal organizations, Washbrook has seen in the emergence of the non-Brahmin Justice Party and the lower caste Self-Respect Party not a *Kulturkampf* between the South Indian Dravidian heritage and the North Indian Aryans, as others have,¹⁰ but rather an economic rivalry among small cliques seeking to advance their position by capturing the patronage of the British administration. Cultural labels served as convenient covers for the combatants, but the labels had little substance. Washbrook has pointed out that the non-Brahmins were not anti-Sanskrit, nor did they individually terminate their friendships with Brahmins. They used the non-Brahmin line only as a political rallying point. Self-interest, not cultural interests, motivated them. Finally, Washbrook has argued that Indian politics flourished only after the British decided, for reasons of economy, to limit their own administrative activities. Until then, the British had been the chief engine of political, social, and economic change in South India; their government, Washbrook has claimed, was “omnipresent in the life of colonial South India.”¹¹ The British decision to cut expenses and withdraw—rather than Indian political victories—ended the block against Indian upward movement in politics.

Baker’s *The Politics of South India, 1920–1937*, carries this analysis forward to the eve of World War II. In Baker’s work, the themes stressed by Washbrook reappear: (1) the British administrative structure invited the collaboration of more Indians, as (2) the British themselves pulled farther back into enclave positions in Madras City and New Delhi and as (3) caste became a convenient rallying point in political organizations. Baker has added a fourth theme: a world depression forced the British to relax their grip for economy’s sake and threatened those Indians who had developed economic interests in the wider world. Baker and Washbrook are at the center of the Cambridge cluster in their revisionist emphasis on self-interest and the importance of local economic issues. They have not, however, emphasized federal issues in the fashion of other researchers of provincial politics.

As the nationalist movement grew, communal demands of religious communities became increasingly important. Francis Robinson’s studies, “Municipal Government and Muslim Separatism in the U.P., 1883–1916,”¹² and

⁹ Washbrook, *The Emergence of Provincial Politics*, 66.

¹⁰ Robert L. Hardgrave, *The Nadars of Tamilnad: The Political Culture of a Community in Change* (Berkeley and Los Angeles, 1969); and Eugene F. Irschick, *Politics and Conflict in South India: The Non-Brahman Movement and Tamil Separatism* (Berkeley and Los Angeles, 1969).

¹¹ Washbrook, *The Emergence of Provincial Politics*, 23.

¹² Robinson, “Municipal Government and Muslim Separatism,” in Gallagher, Johnson, and Seal, *Locality, Province, and Nation*, 69–121.

Separatism among Indian Muslims: The Politics of the United Provinces' Muslims, 1860-1923, extend to Muslims the methodological concerns of the Cambridge group. First, Robinson has tackled geographical linkages, arguing that in the late 1800s and early 1900s the Muslims of the United Provinces could easily have decided to ally with other inland groups, Hindu as well as Muslim, against the political influence of the coastal provinces of Bombay and Bengal. Instead, following the founding of the Indian National Congress in 1885, the most important leader of Muslims in the United Provinces, Sir Syed Ahmad Khan, pushed to protect the interests of Muslims against Hindus rather than the interests of the United Provinces against the coasts. In effect, Robinson has put the onus of the Hindu-Muslim split on the Muslims rather than on the British or on the Hindus or, more abstractly, on the increasing demands of an increasingly democratizing pluralist polity. As the challenge of electoral politics continued, hostility between Muslims and Hindus increased until, finally, issues were seen not only as competition for office and patronage but also, and more starkly, as "Islam in danger." During and after World War I, the religious leadership, the *ulama*, became politically active for the first time, leading to a polarization of Hindu and Muslim interests, which no leader, whether Hindu or Muslim, could long reconcile. Robinson has also seen most of the Muslim leaders as primarily motivated by self-interest, and this contention runs through his prosopographical thumbnail sketches of sixty leaders of the "Young Party," forty-four leaders of the "Old Party," and twelve leaders of the *ulama*. Paradoxically, Robinson has argued that the masses did respond to religious ideology, even though they were led by the self-interested *ulama*.¹³

In addition to local, regional, and communal perspectives on the growth of nationalism, the Cambridge school has also provided two views of modern Indian history written from a national perspective. Of the two authors who have addressed Indian politics at the center, B. R. Tomlinson, in *The Indian National Congress and the Raj, 1929-1942: The Penultimate Phase*, has most clearly followed the Cambridge mainstream. He has described the Congress's transformation from agitational movement to electoral party, critically examining earlier nationalist hagiography. He has emphasized, for example, the skirmishes among Congress politicians for office and position in those provinces deserted by the British. With most of the Cambridge cluster, Tomlinson has pointed to—and censured—Indian self-interest: "politicians were only prepared to fight hard to create or maintain influence in the Congress when such influence would bring them some long or short term benefits." According to Tomlinson, the "archetype" of the provincial congress leader "was much nearer to the modern Congress boss than he was to the idealized freedom-fighter."¹⁴ Tomlinson has also discounted the danger and pain associated with active leadership in Congress. Fines, lathi charges, imprisonment, and torture find no place in his account, but that suffering significantly informed

¹³ Also see Barbara Metcalf's Review of Robinson, *Separatism among Indian Muslims*, in *Journal of Asian Studies*, 35 (1976): 339-41.

¹⁴ Tomlinson, *The Indian National Congress and the Raj, 1929-1942*, 70, 83.

the experience of the victims individually and of the Congress collectively. Further, Tomlinson has devalued the substance of the Congress debates in the 1930s over the crucial questions of socialism, communalism, and regional devolution. He has seen the debates only as a rhetorical cover for factionalism and a betrayal of promises by the Congress. He has also exposed British rhetoric that spoke of devolution even while continuing to hold all real power until 1945, but he has shown more understanding of the British position.

Judith Brown has also examined the nationalist organization from the top down. As her focus and foil, she has taken the political career of Mahatma Gandhi, first his earlier years in Indian politics in *Gandhi's Rise to Power: Indian Politics, 1915-1922*, and then a later period in *Gandhi and Civil Disobedience: The Mahatma in Indian Politics, 1928-34*. Her central questions—which politicians followed Gandhi and why did they accept his leadership?—move away from the study of Gandhi's personality and saintliness. Instead, Brown has investigated Gandhi's political role linking locality, province, and nation. She has sought to understand, first, the political issues in various provinces and localities and, then, how these various strands were—or were not—woven together into a fabric of integrated national politics.

Brown is less dogmatic than the other members of the Cambridge cluster. She has made more allowance for human error, for the unplanned, for the accidental: "Indian political development in the first half of the twentieth century was the product of novel forms of political encounter in which the participants experimented and learned from experience in situations for which there were often few precedents. Ideologies were in the process of creation; patterns and standards of behavior were not clearly defined; nor were institutions deeply entrenched as in longer-established political systems."¹⁶ Brown has chosen political biography, with all of its strengths and weaknesses, to elucidate improvisation in political life. Others, notably Robinson, have presented thumbnail sketches of the local leadership, but these have usually been tailored to make them representative of some particular point of view, paradigms rather than people. Brown, however, has devoted two books (a total of some seven hundred pages) to an understanding of thirteen years in the political life of one individual. She has allowed Gandhi—and those who interacted with him—to have principles, and sometimes to reconsider them; to have interests, and sometimes to sacrifice them; to act with purpose, and sometimes to stumble; and, frequently, to be confused, to retreat, to rethink, and to regroup. She has not oversimplified the complex. She has erred, perhaps, too much on the side of accepting people and their public statements at face value and reporting too much without analyzing—although by voluminous reporting she has often disclosed the internal contradictions in public statements and demonstrated the quarrels of antagonistic leaders. Still, if the other Cambridgeites have been too eagerly contentious, too willing to do battle against their subjects, she may have been too gentle, too willing to suspend critical judgment, too willing to forego even that analysis that hindsight allows to the historian.

¹⁶ Brown, *Gandhi and Civil Disobedience*, 381.

Brown's discursive, nondogmatic probing may reflect, in part, her training with Eric Stokes. Although Stokes is a central figure at Cambridge, this review has not discussed his works inasmuch as they have focused generally on an earlier period, mostly on the first century of British rule through the 1857 Rebellion. A few more recent articles, now available in a single collection, *The Peasant and the Raj: Studies in Agrarian Society and Peasant Rebellion in Colonial India* (Cambridge, 1978), do examine the nationalist period, exploring rural transformation. These essays provide capsule surveys of the literature, detailed analyses of specific local movements, and general speculations on the nature of peasant organization—quite without advocating dogmatic positions. These investigations do not, however, explore the organizational links between local agitations and national political movements—the subject of the current review.

The mainstream of the Cambridge group has seen Indian nationalist organization as inspired by the British: The matrix was British; the leadership was English-speaking and English-trained; the issues were largely set by the British; the political institutions were founded by them; and even the foundation of the Congress itself was inspired by the British. Brown has reversed these judgments: Indians had their own agenda and priorities; the British presence only expanded their scope. In 1920, at the Nagpur Congress, Gandhi moved Indian politics into a new era of mass organization.¹⁶ The movement began to solicit groups that the British had never touched: urban workers, village farmers, untouchables, tribals. The British, whose 1935 Constitution for India allowed only one-fifth of the adults to vote, had not envisioned such wide participation. Moreover, Gandhi did not wait until local factionalism brought new allies to the center, as the Cambridge historians have generally argued; he actively sought his own support. Gandhi did not merely fill in for the departing British. He sought to build a wise, decent, compassionate—paternalistic—movement that could assist the downtrodden in their hour of need. British rule—wishing to hold India (as Gallagher has argued) for its markets, its revenues, its soldiers, and its prestige—had not undertaken these wider aims.

By 1920, and increasingly thereafter, the British sought to minimize government activity, for they could afford no more; the Congress sought to maximize it, for it could afford no less. The sheer growth of the nationalist movement was beginning to require answers to new questions: Which groups should be allowed entrée to national forums and on what terms? What should be the shape of the future government? And what should be the role of voluntary organizations outside and inside that government? The moral authority of the government, as well as its initiative, had passed to the Congress. Gandhi made that clear; Brown has correctly accepted his word.

WHERE DOES THE CAMBRIDGE GROUP fit into the entire framework of contemporary Indian historiography and how shall we understand this whole group of

¹⁶ See Gopal Krishna, "The Development of the Indian National Congress as a Mass Organization, 1918-1923," *Journal of Asian Studies*, 25 (1966): 413-30.

social science-humanists, this collection of imperial historians of India? How do Indian politics look in light of their suggestive work? What are the group's considerable accomplishments, its deficiencies, and its prospects for continuing contributions to the field?

On the positive side of the ledger are the important breakthroughs in asking new questions: on the origins of pluralist politics in modern India; on the developing pattern of linkages among local, provincial, and national political organizations as the driving force sustaining this pluralism; and on political subcontractors beginning to assume functions formerly held by local patrons. Collectively, the Cambridge group has also provided a variety of techniques for investigating these theories of linkage: statistical and ecological analysis, political sociology, prosopography, and political biography. On the negative side are a lack of rigor in testing the social science models that implicitly underlie their research; a failure to exploit materials in Indian languages; a degree of isolation from research taking place in other universities; and, too frequently, a writing style that fails to convey the drama of events. Analysis of these problems reveals an alternative emphasis to that of the Cambridge cluster—though remaining close to the factual material they present.

The Cambridge historians have touched upon at least six significant social science models: (1) pluralist machine politics; (2) patron-client relations; (3) federalism; (4) ideology; (5) diffusion of innovation; and (6) conflict in social change. In each one, greater depth and rigor would have yielded richer insights and more comprehensive understanding of what was occurring in India—and with fewer moralistic judgments. In their stance on pluralist, machine politics, for example, they have taken the major Indian scholarship into account, but they have not discussed these earlier contributors in depth: Tomlinson has cited the work of Rajni Kothari and others on the pluralist nature of Indian politics after independence, but only in order to establish continuities with the pre-independence period, and Washbrook has cited—primarily in order to contest—the earlier cultural interpretations of Eugene F. Irschick, Robert L. Hardgrave, and others on the rise of communalism in South India. Furthermore, the Cambridge scholars have not compared pluralist politics, independence movements, or party formation in other parts of the world at other times under other circumstances. As a result they have been harsher, more critical of the compromises, errors, and self-seeking that have frequently characterized Indian politics.

The Congress political machine that arose across India began to carry out necessary functions, such as flood relief and village extension work, that the government was unwilling or unable to undertake. At least one full generation before independence, Congress carried out some of the “latent functions of the machine,” which Robert K. Merton has described with respect to the United States.¹⁷ Corruption within the growing machine may deserve the condemnation the Cambridge historians have meted out, but it also functioned to grease the political machine. Corruption has sometimes been seen as private

¹⁷ Merton, *Social Theory and Social Structure* (New York, 1957).

compensation for political work that public office did not adequately reward, as James C. Scott has claimed for other newly developing societies generally and Seymour Mandelbaum has claimed for the pre-industrial United States specifically.¹⁸ The difficulties of organizing the disparate groups in India might also have received more sensitive evaluation had the Cambridge historians compared India with other countries that had similar problems, following the model for new states provided by Clifford Geertz.¹⁹ Finally, the group might have done more to explicate the various layers and styles of constituent groups—secular, religious, caste—and the difficulties and successes of the Congress in accommodating them within a single framework. (Christopher Bayly and others have raised this issue, but none has analyzed it sufficiently.) In “India’s Political Idioms,” for example, W. H. Morris-Jones has suggested that variant political styles of different interest groups derived, at least in part, from the different values of the groups.²⁰

Similarly, some of the authors, Christopher Bayly most forcefully and consistently, have discussed the patron-client relationship in the traditional Indian social structure, but they have not pursued the transformation of this relationship into the patronage politics of a political party. Patron-client interdependence has been recorded throughout the world, and several important studies examine it in India.²¹ Within a locality the patron-client relationship linked dyadically a supplier of goods and services—land for tilling, jobs, protection—with a recipient, who in turn supplied deference, a share of the crop, labor, and military assistance when necessary. Although this traditional interdependence still endures in modern India, relationships of much greater geographical scope have grown up alongside of, and have sometimes displaced, it. These new relationships have been centered on the dispensing of the benefits of the growing political system: jobs, possibilities of office, contracts for government projects, and help in negotiating with bureaucrats, officials, and the courts—all in exchange for votes. Sometimes individuals captured control of these benefits and dispensed them very much in the older patron-client, dyadic mode—but over a wider territory. At other times a “brokerage” function developed in which a middleman served to connect the supply of benefits from the political system with those who wanted them. The broker did not create the benefits, as the patron did, but he had a degree of control over access to them. In many areas the greater abundance of benefits coming from the political system established alternatives to the older patron-

¹⁸ Scott, “An Essay on the Political Functions of Corruption,” *Asian Survey*, 5 (1967): 501–23; and Mandelbaum, *Boss Tweed’s New York* (New York, 1965).

¹⁹ Geertz, ed., *Old Societies and New States: The Quest for Modernity in Asia and Africa* (New York, 1963).

²⁰ Morris-Jones, “India’s Political Idioms,” in C. H. Philips, ed., *Politics and Society in India* (London, 1963), 133–54.

²¹ For a very useful general reader, see Steffan W. Schmidt et al., eds., *Friends, Followers, and Factions: A Reader in Political Clientelism* (Berkeley and Los Angeles, 1977), especially James C. Scott, “Patron-Client Politics and Political Change in Southeast Asia,” 123–46, reprinted from the *American Political Science Review*, 66 (1972): 91–113. And, for studies particularly useful for India, see Paul R. Brass, *Factional Politics in an Indian State: The Congress Party in Uttar Pradesh* (Berkeley and Los Angeles, 1965); Frederick G. Bailey, *Politics and Social Change: Orissa in 1959* (Berkeley and Los Angeles, 1963); and Myron Weiner, *Party Building in a New Nation: The Indian National Congress* (Chicago, 1967).

client relationships and, in some cases, swamped them. (In other areas, of course, the new political system had no substantial impact.) The result was to extend local linkages, through political bosses, into much larger systems. The lure was usually tangible benefits rather than ideological beliefs—but that had also been true of the older patron-client relationships. This developmental, comparative perspective on the growth of political patronage helps explain the process of machine building with less of the moral judgmentalism than that which frequently marks the work of the Cambridge historians.

A federal system, only hinted at by several Cambridgeites, also began to characterize Indian politics. Gordon Johnson has perhaps gotten closest to this concept in his study of the interactions of provincial Bombay and central Congress leaders. Reference to studies in federalism might have provided, once again, an understanding of the need to accommodate differences, to quarantine regional polarizations, to mute quarrels, to dampen radicalism, and to compromise.²² These characteristics—at odds with the characteristics of more ideological political systems—are the essence of federalist solutions. India fits that model, and India might be seen most fruitfully in those terms rather than in the Cambridge group's perspective as a failure of ideological politics.

On the role of ideology, too, the Cambridge historians might have shown more understanding. Anil Seal correctly cautioned that, in "catching" Indian politicians "out at their tricks and dwelling on their inconsistencies, historians are simply playing a game at which administrators used to excel."²³ Yet the Cambridge scholars have frequently set themselves up as wiser than the politicians they have discussed, choosing a one-sided definition of ideology that has allowed them from the comfort of the Cambridge library to do battle against the Indian politicians of a half-century earlier. The seminal thinker on modern political ideology, Karl Mannheim, noted fifty years ago that ideology can be a "conscious political lie," but more deeply it is "the outlook inevitably associated with a given historical and social situation and the *Weltanschauung* and style of thought bound up with it."²⁴ Thus, the historian can point out lies in a publicly stated ideology; they are there to be seen. But the more useful and more difficult task is to explicate the "historical . . . situation and *Weltanschauung*" that produces ideology. Anil Seal understood that directive, and so have Christopher Bayly and Judith Brown in their research. Others, perhaps out of a desire to strengthen their revisionist interpretations, have generally been less careful.

Another model that the Cambridge cluster has implied, but not fully exploited, is the geographical model for the diffusion of innovation. Geographers such as Torsten Hägerstrand and Allan R. Pred have shown the utility of such models in historical research, noting that innovations spread faster in some locales than in others and using that information as the point of

²² See Daniel J. Elazar, "Federalism," *International Encyclopedia of the Social Sciences*, 5 (1968): 353–67.

²³ Seal, "Imperialism and Nationalism in India," 25.

²⁴ Mannheim, *Ideology and Utopia: An Introduction to the Sociology of Knowledge*, trans. Louis Wirth and Edward Shils (New York, 1936), 125.

departure for raising useful questions about the society.²⁵ The Cambridgeites have charted the nationalist organization and have loosely traced its spread across locality, province, and nation. They could have applied this diffusion theory more rigorously and, therefore, more productively to many aspects of political innovation. They have perhaps too quickly credited the British for the thrust toward political change. They have generally—except, perhaps, for Christopher Bayly—discounted business and religious networks in fostering national unity. Tracing the actual progress of the nationalist organization in its diffusion across the subcontinent over time might have provided greater insight into the significance of nonpolitical networks. Furthermore, the nationalist organization had significant geographical anomalies. Although the British centers were Bombay, Calcutta, Madras, and New Delhi, the nationalist capitals were more frequently Poona, Ahmedabad, Allahabad, and Wardha—the homes of the leading nationalists. In some ways Gandhi's position in particular was like that of the former Mughal emperor—the capital of the country was wherever he was. A fully elaborated diffusion model might have analyzed the significance of these geographical idiosyncracies.

A sixth theory implicit in much of the work of the Cambridge group is the conflict model of social change: New issues arising in society polarize groups and out of the conflict of these forces new social forms arise and new balances are struck. Karl Marx, perhaps the most famous proponent of this approach, narrowed the realm of conflict to the issue of economic class, but any issue—economic, social, political, intellectual—can form the basis for polarization.²⁶ The Cambridge historians have, unfortunately, often accepted that narrower focus on economic self-interest. David Washbrook and B. R. Tomlinson have been most explicit in delimiting the focus, but Francis Robinson has also seen even in "Muslim communalism" the "religious" issue as a by-product of economic self-interest. Public political debate by intellectuals has been presented as a cover for the real issues, which are economic; the publicists are hired guns of the economic interest groups. This narrowing of the range of conflict has fostered scholarly cynicism and has allowed these historians to score some easy points. But it obscures the vast array of social, religious, and cultural issues that began to find their way into the political arena.

The Cambridge group has unnecessarily limited not only its vistas of theory and comparison but also apparently its use of Indian language sources. Only Christopher Baker and Christopher Bayly have relied significantly upon materials in Indian languages. Only Bayly, Judith Brown, and Anil Seal in his earlier work have suggested a willingness to see Indian events primarily through the eyes of the Indians themselves. Bayly's single urban area, Allahabad, is compact enough to allow the use of materials from all points of view; Brown's Gandhi is so titanic a subject that sources from every perspective

²⁵ Hägerstrand, *Innovation Diffusion as a Spatial Process* (Chicago, 1967); and Pred, *Urban Growth and the Circulation of Information: The United States System of Cities, 1790-1840* (Cambridge, Mass., 1973).

²⁶ Ralf Dahrendorf, *Class and Class Conflict in Industrial Society* (Stanford, 1959), and *Essays in the Theory of Society* (Stanford, 1968).

abound; and Seal's research design encouraged a broad-based analysis. But the others have relied primarily on contemporary British assessments of events. It is little wonder that the historians' views so often parallel those of the British administrators whose records they use. B. R. Tomlinson perhaps has an out: He has used Rajendra Prasad's papers to show, through Indian eyes, a disgust with Indian avarice in politics. But Prasad must be taken with a grain of salt. He wrote during the heat of political battle and amid the frustrations of everyday political organizing. Despite his disenchantment, he never became discouraged enough to give up. He voted with his feet to stay with the struggle.

The Cambridge group has also seemed to slight important insights of other scholars studying India. The Australian National University, for example, in 1966 sponsored an international seminar on the Rowlatt Satyagraha of 1919, the protest movement that catapulted Mahatma Gandhi to national leadership.²⁷ The papers from that session analyze one political event in six regional settings and give a sense of the correlation of local issues, conditions, and leadership across the nation. The focus on a single major event gives coherence, yet it allows regional particularity to emerge. Such an approach might have been useful at Cambridge as well. Another approach to the study of regions in India appeared in two widely interdisciplinary seminars at Duke University, the first in 1966 and the second in 1973. In these seminars, the papers from which were published under the auspices of Duke's Program in Comparative Studies in Southern Asia, the focus of attention was not on the integration of region with nation but rather on modes of conceptualizing Indian regions and their significance in the history, anthropology, and politics of the country.²⁸ Such studies help maintain a perspective on locality, province, and nation. After all, the British were not the first to rule India through its regions. Indeed, they adopted the administrative network first worked out by Todar Mal, minister to Akbar, the sixteenth-century Mughal emperor. These broader perspectives, frequently drawn from within the society's own view of itself, enrich the context for understanding the integrative process in the modern nationalist movement.

Finally, a word on style and the historian's craft generally. The Cambridge historians have chosen the social scientist's mode in emphasizing the application of social science models and the disaggregation of data. They have mostly eschewed the sense of drama, of sweep, of narrative exposition. Anil Seal, to his credit, has not. He has conveyed passion even when pinpointing detail. Except for Gordon Johnson, Seal's associates mostly have not. They have cast the struggle for freedom almost entirely in terms of self-seeking, and, perhaps for that reason, their writing about it is for the most part turgid. Some balance is needed between the detailed, careful methodological research

²⁷ Ravinder Kumar, ed., *Essays on Gandhian Politics: The Rowlatt Satyagraha of 1919* (Oxford, 1971).

²⁸ Robert Crane, ed., *Region and Regionalism in South Asian Studies: An Exploratory Study* (Durham, N.C., 1967); and Richard G. Fox, ed., *Realm and Region in Traditional India* (Durham, N.C., 1977).

of the social sciences and the sweep of passion and drama that bring to history excitement, value considerations, enlightenment—and entertainment.²⁹

I PROPOSE AN ALTERNATIVE SYNTHESIS built largely on the foregoing materials but using a different perspective. An attempt to comprehend the Cambridge scholars in tandem with scholarship in Indian history elsewhere and with social science theory provides a richly suggestive mix of ideas: A modern political system—pluralist, federalist, machine-dominated—began to germinate in some areas of India at least as early as the 1870s. The system not only included parties based on political ideology but also comprehended issues that had previously been expressed almost solely in private lives and segmented communities—issues of religion and caste as well as those of governance and class. A new political system had to encompass a population whose fundamental perspectives on man and God spanned the whole spectrum from beliefs in the inequality of all individuals and groups (and gods) to radical new beliefs in equality. All of these differences compounded the geographic diversity of India's people, customs, languages, and local patron-client relationships. Increased communication brought all of these groups into contact for the first time and, thus, demanded new patterns for public life. How could accommodation be found? That was the central question underlying the formulation of a modern polity. The significant innovation, then, was not so much nationalism as it was secular public life. This new dimension of public life spread across the nation by various means: forceful extension within the British framework of government, schools, jobs, and the press, all of which invited competition among various groups; increasing transactions in business and printing, including religious propaganda; and geographical migration of individuals and groups, which forced contact, especially in the large cities, among groups formerly alien to one another.

The form and content of the new public accommodations were the subject of intense conflict. Conflicts arose over places in government, schools, and jobs; over market dominance; over the language of public life; over the place and style of religion and the role of the religious establishment; over the distribution of tax monies; over the degree of equality among people; over disparate beliefs concerning the mission of Indian civilization; over British rule in Turkey—over innumerable issues, local, provincial, national, and international. As social density increased, so, too, did the issues in conflict; more and more people found their lives—previously settled and private—now disturbed by new public activities and new ideologies. The new ideologies expressed great ferment and were accompanied by the recruiting of disciples. The ideologies were, in part, no more than pompous rhetoric and even lies, but, in part, they were attempts by more thoughtful people to put into words and programs their perceptions of the changing political, social, economic,

²⁹ For some recent comments on this problem in general, see M. I. Finley, "'Progress' in Historiography," *Daedalus*, 106 (1977): 125–42.

and religious realities. Indians needed such formulations both to clarify their own thinking and, by publicizing their views, to win allies who would help create the world in that image. Sometimes they used the ideological rhetoric of public welfare to mask private agendas. Sometimes they spoke of ideas more fixed and decided than later proved to be true. Sometimes patrons of journalist-ideologues had excessive influence over intellectual life. But, for many intellectuals and publicists, ideological statements represented attempts to define their situation and articulate the status of their group and their nation in a time of great flux.³⁰

Part of the fluidity in public life in India came from new British policies and from technological change. Much, however, came from the very diversity of people and interests in India. Pluralism became the dominant form of public political life. A wide variety of groups with shifting memberships entered politics, groups founded on very different bases of cohesion—caste, class, language, occupation, religion, family, region, and sect. Since people had many interests, they often belonged to more than one group and shifted the emphasis of their participation from time to time depending on the salient issues of the day. But, generally, people were committed to maintaining the diversity of their particular identities rather than to merging all together into a homogeneous Indian nation. Indeed, that desire to preserve variety inspired many of the conflicts.

In one sense, the growth of a political center impinged on groups at the periphery, forcing them to take stands on new, previously remote issues. By the same token, it offered new sources of wealth and power that eroded the bonds of older, more localized patron-client relationships and frequently replaced them with the new patronage of the political machine. The newly building political structure, both British and Indian, took over many of the functions for which clients had sought patrons, including (though imperfectly) the provision for police and judicial powers. The new government could dispense patronage far beyond the means of local magnates. This patronage allowed the Congress politician to become a new political boss, a kind of patron of patrons. It also created the new job of “brokering” those who wanted favors and positions with the resources of the government. Some patrons retained their clients in place; others had to modify their style and functions in order to maintain their position, primarily by becoming brokers in the larger system; and some were displaced by new men better connected to machine patronage and more adept at using it.

Religious evangelism also began to create new networks and senses of identity. More efficient transportation and communication systems allowed increasing fulfillment of the vision of an India unified in holiness. Mahatma Gandhi's saintly presentation tapped that chord and even the secular Jawaharlal Nehru felt it. Business, too, began to form nationwide networks through the expansion of individual firms, the linkage into organized systems

³⁰ For a useful collection of ideological statements on a wide range of topics, see William Theodore de Bary, ed., *Sources of Indian Tradition* (New York, 1958).

among firms, and the growth of an industrial and urban labor force. Politics was not alone in creating new networks.

Finally, as Indian localities and groups began increasingly to mesh with one another, the countervailing force of federalism allowed them to keep significant levels of autonomy and identity. It frequently permitted local patrons to retain power and authority in their small sectors; it enabled and even encouraged localities and provinces to cope in their own ways with local problems, yet at the same time to find satisfactory linkages with national groups and policies. The federal principle was a cushion against the increasing density of social interaction that government and modern communication otherwise fostered. Federalist accommodation encouraged Indians to mute their battles, to isolate local struggles, and to find compromises at the national level among provincial and local interests.³¹ It discouraged ideological politics and encouraged the machine, pluralism, and patronage-based politics. These patterns began to emerge almost a century before independence. The Cambridge monographs, although their emphases frequently lie elsewhere, clearly illuminate this historical dimension.

WHAT OF THE FUTURE of the Cambridge group? They are comparatively young. Anil Seal is just forty-one. Only John Gallagher is older. And they are now all moving off in their own directions. The central question of the relationship of subnational units to the nationalist movement no longer dominates their work. Christopher Bayly continues to work on the urban system and urban elites in northern India in the eighteenth and nineteenth centuries; Judith Brown is beginning to prepare a textbook on modern Indian history and also is studying religious attitudes among Hindus in urban India today; Gordon Johnson is going further back into the eighteenth-century history of western India; Francis Robinson is investigating the northern India Muslim community on the basis of local records recently made available to him; B. R. Tomlinson is examining the history of European business firms in twentieth-century India; and David Washbrook is studying the history of modern Andhra. The nationalist movement is no longer at the center of their attention. As they have moved away from that common ground at Cambridge and that common era in their academic and intellectual development, their interests have diverged. The Cambridge cluster has fissioned. Although its collective contribution will have impact on Indian historiography—and on the study of pluralist politics in other settings as well—for years to come, the “academic generation”³² that was the Cambridge group has now moved on to new and separate paths.

³¹ For the virtues of the continued diffusion of power in modernizing societies, see Robert Nisbet, *The Quest for Community* (rev. ed., London, 1969).

³² I thank Judith Brown for this phrase.

Reviews of Books

GENERAL

ALFRED SOHN-RETHEL. *Intellectual and Manual Labour: A Critique of Epistemology*. Translated by MARTIN SOHN-RETHEL. Atlantic Highlands, N.J.: Humanities Press. 1978. Pp. xiv, 216. \$19.00.

DAVID-HILLEL RUBEN. *Marxism and Materialism: A Study in Marxist Theory of Knowledge*. (Marxist Theory and Contemporary Capitalism.) Atlantic Highlands, N.J.: Humanities Press. 1978. Pp. x, 199. \$20.00.

Alfred Sohn-Rethel's *Intellectual and Manual Labour* embarks on a strange, if not incoherent, project. The author believes that if society is to control technology and if mental and manual labor are to be integrated—two laudable Marxian goals—then it is necessary to demonstrate not only the social roots of science but the social prerequisites of intellectual abstraction itself. Turning to Marx's discussion of exchange in *Capital*, Sohn-Rethel seizes on what he considers the "abstractness" inherent in commodity exchange. This consists, primarily, in the fact that in the market-place the exchange of goods is separated ("abstracted") from their consumption, and money emerges as the bearer of an abstraction, namely, exchange value. These "abstractions" are carried out unconsciously, but they nonetheless generate such nonempirical concepts as time, space, matter, motion, and quantity with which the intellect, especially in science, must work. Kant, to whom the author compares himself, thought such concepts were supplied *a priori* by the mind as it organized sense experience; Sohn-Rethel sees them as a result of the commodity process. The intellect, however, is not aware of the social genesis of its basic concepts and so views itself, incorrectly, as self-sufficient and independent of manual labor—hence, the unhappy schism between mental and physical work and the alienation of science from society.

Regardless of what one makes of Sohn-Rethel's intended enterprise, one will search his book in vain for any conceptual or procedural clarity—or

indeed for anything resembling argument. His theme is frequently voiced, Marx is quoted, and many sentences are strung together, but the reader is continually hard pressed to understand what point the author thinks he is making or how it supports his thesis.

By contrast, David-Hillel Ruben's *Marxism and Materialism* is a model of clarity and competence. Although Ruben's book is directed to those in the Marxist tradition, he is evidently at home with the literature and sensitive to the professional standards of contemporary non-Marxist, Anglo-American philosophy.

Ruben contends that if Marxist materialism is to be consistent, then it requires a correspondence theory of knowledge. He returns to Kant in order to make his case, showing how Kant's belief that all knowledge presupposes the interpretive activity of thought cannot be squared with his commitment to the existence of objects independent of the mind. Hegel and Feuerbach grasped opposite horns of this dilemma, and Marx followed the latter in adopting a materialist perspective. As a materialist, though, he must also hold the epistemological thesis that truth consists in the correspondence of thought to the world.

The author then turns, appropriately, to arguing that a correspondence (or reflection) theory—although considered by many Marxists to be fatally undialectical—is, when suitably stated, acceptable from the Marxist point of view. In his final chapter Ruben reviews Lenin's attempt to formulate such a theory in *Materialism and Empirio-Criticism*, concluding that, although Lenin's heart was in the right place philosophically, he confused a reflection theory of knowledge with an untenable reflection theory of perception.

Ruben is precise in his terminology and careful to distinguish related but distinct philosophical positions. He discusses Kant, Hegel, and Marx with intelligence, and his basic thesis is sensible and well argued. One might wish, however, that the author had been more ambitious. He paves the way for a materialist theory of knowledge but does not actually develop one; nor does he attend to the

problems that contemporary non-Marxist philosophers have raised for correspondence theories of the type he suggests. These are big tasks. Perhaps Ruben will be kind enough to undertake them in the future.

WILLIAM H. SHAW
University of Tennessee,
Nashville

theory sufficient as praxis? After all, it was not Marx with his theory, but Lenin and Mao with their praxis who did succeed in changing the world. And there is a price to be paid for revolution. That is the dilemma.

DONALD M. LOWE
San Francisco State University

DICK HOWARD. *The Marxian Legacy*. New York: Urizen Books. 1977. Pp. xv, 340. Cloth \$15.00, paper \$6.95.

Dick Howard was a member of the New Left who, in the course of the 1960s, moved from a phenomenological-existential stance to the Marxist unity of theory and praxis. Marxist theory enabled the New Left to discover that the locus of political action was that sphere of the everyday that is civil society. But the disintegration of the movement in the 1970s forces Howard to ask anew, "what is the political?" as the foundation for revolutionary praxis. Marxism is not a closed system. Rather, it is a project that mediates philosophy and the world. In a different situation, one must rethink the Marxist project. Therefore, Howard goes back to interrogate those seminal Marxist thinkers who had similar concerns about the application of the Marxist unity of theory and praxis to their situations—namely Luxemburg, Bloch, Horkheimer, Habermas, Sartre, Merleau-Ponty, Claude Lefort, and Cornelius Castoriadis.

This book is neither intellectual portraiture nor history of ideas but a personal, critical study of these eight thinkers in order to renew the Marxist unity of theory and praxis. "I will be starting from the premises of Marx, adapting his method to the present, and then using the structure of that present—including the lesson of the New Left—to criticize Marx. This does not lessen the importance of Marx; nor should it be interpreted within a Manichaean friend-foe context" (p. 20). Howard is honest and forthright in his criticism of the positivism, elitism, and bureaucratization inherent in Marxism. Nevertheless, he believes that Marxism is still the only philosophy to open a horizon in our world. And he is acute in his evaluation of the eight Marxist thinkers.

The author concludes that "Castoriadis and Lefort have opened up the dimension of the political which had for too long been taken for granted by the revolutionaries. It is to the structure and suppositions of this political project that attention must now be turned" (p. 301). This book is in itself an admirable project. And, for Howard, the quest for the political project is the next task.

But there is a gnawing question: Is the project of

ARTHUR L. STINCHCOMBE. *Theoretical Methods in Social History*. (Studies in Social Discontinuity.) New York: Academic Press. 1978. Pp. x, 130. \$10.00.

This is a book by a sociologist about the relationship between general theories and historical studies that challenges the conventional sociologist's notion of theory and data but confirms the working instincts of the ordinary historian. It is a "personal" book in that it is not a systematic treatise on the subject or even a review of the literature but a series of case studies on the writings of Trotsky, de Tocqueville, Smelser, and Bendix to discover how theorists actually do historical studies as opposed to what they say they do. The book is also personal in that the introduction and conclusions are written in an argumentative and sometimes didactic style that has overtones of classroom intellectual provocation.

Arthur L. Stinchcombe's primary contention is that causal theory, and especially theories of epochal historical movements or change, should be derived from empirical studies and from the detailed construction of analogies between instances, not deduced from general theoretical suppositions. Thus he analyzes the strategies of selected authors to see how they go about theory making regardless of their presumed theoretical commitments. In an excellent chapter on Trotsky's *History of the Russian Revolution* and de Tocqueville's *Old Regime and the French Revolution*, Stinchcombe shows how Trotsky analyzes the revolution in terms of the exercise of political authority and the situations and dispositions that channel the choices people make to sustain or replace an existing regime. In the *Russian Revolution* the analysis focuses on the breakdown of the authority of the tsarist regime and on the way in which competing organizations, including the state and the Bolshevik party, attempted to become the channel through which solutions to political and economic problems could be found. In his analysis of Smelser's *Social Change in the Industrial Revolution*, Stinchcombe finds a similar pattern of historical change described in terms of an initial functionally integrated situation, a process of change in which working-class families adapted household and family needs to the exigencies of a new factory system, and the cumulation of choices which, once made, built up a new

end situation. Again, Stinchcombe focuses on the choices and actions that mediate the overall process of social differentiation. As he says in conclusion: "The causal forces that make systematic social change go are people figuring out what to do. Instincts stay the same; functional prerequisites remain prerequisite; the cognitive content of people's minds changes. It changes in particular to take account of a new situation, so that cognitive change can have a cumulative and self-sustaining quality. What we have to study to understand history is how structural forces cause people to change their notions of what kind of situation they are in, and to sustain those new notions sufficiently long to build them into institutions that in turn sustain them. . . . Thus the elementary analogies, out of which wide ranging general concepts ought eventually to be constructed, are analogies between the thoughts of people about their situations, not analogies between structural dilemmas or functional strains of systems" (pp. 117, 121).

Though these conclusions may seem familiar and even obvious, they are important for several reasons. Though Stinchcombe's critique is directed against misuse of sociological methods, he still stands not only for direct confrontation with the data but also for the construction of theories that reveal the causal structure of situations and are capable of being generalized to appropriately analogous situations. Secondly, in his concept of change, the focal point is human action and choices—the responses people make to their situation, and one may add, to their ideals and hopes. Stinchcombe denies the value of conventional explanations in terms of relations between abstract structures, like "economic" and "political" forces, or the logic of ideas, or functional requisites. From his sociologist's perspective Stinchcombe is trying to bridge a gap that has plagued historians for a century—the gap between history seen as the actions of humans in a sequence of events and historical change seen as the result of basic structures and developments that are not subject to human intervention and choice. Though the perspective is unusual and individual, the lesson is worth learning from yet another point of view.

IRA M. LAPIDUS
University of California,
Berkeley

MARTIN WIGHT. *Power Politics*. Edited by HEDLEY BULL and CARSTEN HOLBRAAD. Rev. ed. New York: Holmes and Meier, for the Royal Institute of International Affairs. 1978. Pp. 317. \$22.50.

Thanks to the efforts of the editors, who have completed Martin Wight's manuscript, we have

here a splendid entertainment that many lively and professional historians will not want to miss. This is a delightful book to browse through, contemplate, refute, and enjoy more than once.

Among the resplendent historical allusions, wit, and tight reasonings, Wight has given us a number of explicit and suggestive definitions of the sort one rarely gets and always needs: "alliances," "balance of power," "collective security," "diplomacy," "disarmament," "expansionism," "intervention," "power politics," "great, middle, minor, and world powers," "states-system" (about which a book by Wight was posthumously published in 1977), and "war."

One should not review this sort of book without examples; here are too few. "The doctrine that there are no valid members of international society save those born of national self-determination triumphed when in the shock of the First World War, the military multinational empires of Eastern Europe—German, Habsburg, Russian and Ottoman—collapsed. The Versailles Settlement was the final victory in Europe of the French Revolution over the Holy Alliance" (p. 85). "A Foreign Minister is chosen and paid to look after the interests of his country, and not to be a delegate for the human race" (p. 95). "Closely bound up with the idea of 'honour' is the idea of 'prestige'. Honour is the halo round interests; prestige is the halo round power" (p. 97). "It may be noted, as a melancholy law of coalitions, that they are designed to avert the last war. If in 1914 Britain had had the degree of public commitment to France which she had after 1936, the war of 1914 might have been avoided. If in 1938–9 Hitler had been confronted with the Brussels Organization of 1948, he would have been deterred" (p. 134). "If your neighbour is your 'natural enemy', the power on the other side of your neighbour is your natural ally" (p. 158). "Thus American policy after 1947 was seeking at the same time to restore the balance of strength against Russia by rearmament, and to be able, in Dulles's words, to 'negotiate from a position of strength'. This is indeed a fundamental conundrum of international politics" (p. 175). "The alternatives to the balance of power are either universal anarchy or universal dominion. A little reflection will show that the balance of power is preferable to the first; and we have not yet been persuaded that the second is so preferable to the balance of power that we shall submit to it" (pp. 184–85).

Other matters arise from this book. It seems to me that those of us who read, buy, write, use, and, especially, review books must shortly insist that publishers not only hold to acceptable scholarly standards by including informative indexes and footnotes, which has been done here, but must also

resist foolish economies, which has not been done here, that omit bibliographies and biographical information about the qualifications of editors and authors within the end pages. Publishers should not be permitted, without protest, to assume readers have the kinds of information that only insiders possess.

F. P. KING
Smithsonian Institution

PAUL THOMPSON. *The Voice of the Past: Oral History*. New York: Oxford University Press. 1978. Pp. xi, 257. Cloth \$10.95, paper \$5.50.

Out of England comes the first book to combine a theory of oral history, the technical processes involved, and a road map of where oral evidence fits into the landscape of Western historiography. Alive with concrete examples, this small volume is ideal for historians (and foundation officers) who are grappling with the relative value of oral history as a primary source, for teachers in search of a basic text that will both inform and provoke lively classroom discussion, and for practitioners in other disciplines that use oral sources, particularly anthropologists and sociologists. Ten pages of model questions in the appendix will be invaluable as concrete suggestions for novice interviewers and as a check list for veterans; the fullness and the annotations in the fifteen pages of bibliography alone are worth the price of the book.

Paul Thompson, a sociologist at Essex University, is an articulate spokesman for the view that "a history is required which leads to action: not to confirm, but to change the world." With that as his premise, he places oral history in a central position: "The nature of most existing records is to reflect the standpoint of authority. . . . Witnesses can now also be called from the underclasses, the unprivileged, and the defeated. . . . In so doing, oral history has radical implications for the social message of history as a whole."

Whether the readers agree that history should be aimed at social action or not, they will find Thompson making a convincing case for oral history in elitist as well as the predictable, demotic subjects: political history as well as lives of oppressed minorities; business and industry as well as labor; military and diplomatic as well as urban and family. For the orthodox historian, Thompson details example after example in which oral history has shown that existing documentary records are both insufficient and misleading.

Reminding us that reality is complex and many-sided, he also illustrates that a series of interviews with a wide selection of participants is particularly suited to capture this untidiness of the past. One of

his numerous examples is taken from projects that trace the development of scientific theory: only oral sources capture the dead ends, misunderstandings, accidental discoveries, and the setting of "acute rivalries." Similarly, in labor history, it is the oral interviews that get beyond the heroics to "the more humdrum, confused reality and different standpoints within the rank and file."

For many, the newness of tape-recorded oral history makes it awkward to incorporate into traditional methodology, but oral history per se is as old as history itself. Thompson takes the reader on a quotation-filled trip through the history of oral evidence, calling on Herodotus in the fifth century B.C., thence through Lucian, Bede, Voltaire, Marx, Engels, Henry Mayhew, Macaulay, H. H. Bancroft, Jules Michelet, and on into the twentieth century to Thompson's favorite, Beatrice Webb. (Beatrice Webb also stars with present-day historians in advice and counsel in the chapter on interviewing techniques.) Perhaps there is a social message in the fact that one who admits to a "socialist perspective" (p. x) has written this for all historians, particularly the orthodox, the conservative, the elitist—Thompson's adjectives for American historians; the irony is that this exposition on oral history, the most complete to date, may well hasten the day when historiography, in a sublimely reactionary swing, will go back to the old method of relying on oral sources to reconstruct the past.

WILLA BAUM
AMELIA FRY
Regional Oral History Office
The Bancroft Library
University of California,
Berkeley

STANLEY SOUTH. *Method and Theory in Historical Archeology*. (Studies in Archeology.) New York: Academic Press. 1977. Pp. xxiii, 345. \$16.50.

STANLEY SOUTH, editor. *Research Strategies in Historical Archeology*. (Studies in Archeology.) New York: Academic Press. 1977. Pp. xxvii, 345. \$19.50.

The last several decades have seen the emergence of historical archeology, a new field that focuses on the archeology of sites that postdate the expansion of European populations after the fourteenth century. Since these sites were occupied by peoples of literate tradition, they are distinguished from pre-literate or prehistoric sites. Because of the time period it deals with, historical archeology uses information and data that have traditionally been assigned to the realms of either history or anthropology. In *Method and Theory in Historical Archeology*, Stanley South does not dwell on the stormy court-

ship and marriage of these parent disciplines. He does, however, present some definitions on how the offspring should be reared.

One of the developmental difficulties of historical archeology is that its mixed heritage requires that practitioners be skilled in the use of history, rooted firmly as it is in the humanities, as well as anthropology, a social science. South's book presents a strong and persuasive argument that the methodology of historical archeology must rest on the scientific recovery and analysis of archeological material in order to elucidate the preserved patterning of human behavior responsible for and retrievable from the archeological record. As illustration, South defines several patterns of refuse disposal typical of the colonial period on the Eastern seaboard. Testing reveals that, for example, the British colonials who settled in this area during the eighteenth century exhibited a highly similar pattern irrespective of locality, site function, or class of occupants. This pattern, called the "Carolina Pattern," may be contrasted with other disposal patterns attributable to French-and-Indian-War sites belonging to the "Frontier Pattern" of disposal.

It is conceivable that historians studying these periods may find these newly defined patterns of dubious significance in ordering and interpreting the tier of historic fact. South's conclusions, however, go well beyond the importance of these patterns *per se*. This book, as well as a companion anthology of historic site studies, *Research Strategies in Historical Archeology*, edited by South, reveal that many aspects of human behavior in the past may never appear in the literate record. Such behavior was the result of unarticulated and/or even unconscious adherence to culturally determined values and beliefs translated into things and events. If this proposition is correct, and these books present convincing evidence that it is, then historians should note that it calls into question two widely held assumptions. These are: (1) the literate record repositied in books and documents contains the best evidence for understanding the past; and (2) the surviving artifactual record of the past is, therefore, of dubious use for historians in their pursuit of historical understanding. Indeed, South and his colleagues demonstrate that artifactual evidence can reveal significant information concerning both the facts of history and the historic process and that this information is often not available in the literate record.

Historians should be warned that these books are weighted with the details of archeology. Anthropologists will be disappointed at the severe regional bias of this book. And readers of any academic preference will probably agree that the diagrams meant to be "heuristic devices" and la-

beled "chicken chart," "pig chart," and so on might have been more aptly included under the common label "corn chart."

These criticisms aside, *Method and Theory in Historical Archeology* is a landmark book. The introductory chapter should be required reading for those interested in the theory of historical archeology and its relationship to history and anthropology. The well-illustrated chapters that follow systematically define and test the proposed Carolina and Frontier Patterns of refuse disposal. The chapter exploring analytical techniques examines both kitchen artifact patterns and material inventory patterns and includes an insightful summarizing essay, "The Flax Hackle Example." The following chapters discuss the importance of quantification in pattern recognition as well as methodological considerations in collecting empirical data in the field. A final chapter on the archeologist's responsibilities in cultural resource management is well done but belongs in another book.

Research Strategies in Historical Archeology, edited by Stanley South, is dedicated to "those who religiously believe that the archeological record reveals far more than merely what happened, when and to whom." Indeed, the twelve studies collected in this book illustrate that this possibility has already become a reality. As perhaps the most exciting example, we can turn to the fine study by Kenneth E. Lewis "Sampling the Archeological Frontier: Regional Models and Component Analysis." By placing his examination of the archeology of the site of Camden on the eighteenth-century Carolina frontier in a theoretical context, he examines the concept of frontiers, thus revealing a specific process of sociocultural adaptation by colonizing societies to frontier situations. This perspective moves us a long way from the repetitive idiosyncratic understanding of particular frontiers to understanding the frontier process.

These two books are significant. They not only illustrate the best of interdisciplinary research, but also show that it produces substantive results. Perhaps more importantly, they illustrate that the whole of history is more than the sum of its documents and artifacts.

CHARLES E. CLELAND
Michigan State University

C. R. BOXER. *The Church Militant and Iberian Expansion, 1440-1770*. (Johns Hopkins Symposia in Comparative History, number 10.) Baltimore: Johns Hopkins University Press. 1978. Pp. xi, 148. \$9.50.

This slim book consists of four lectures delivered at Johns Hopkins University and published with additional notes. The subject is huge, requiring a

team of experts and several volumes for adequate treatment, but C. R. Boxer confines himself to only a few aspects of a vast process.

With his usual synthesizing skill Boxer vaults the centuries and wanders about from Mexico to Africa, India, and China and back again. An effective opening chapter on race relations exposes a key weakness of Iberian missionary efforts: the failure to train an indigenous clergy. The chapter closes with some crisp pages on the Vatican's failure to condemn African slavery and concludes that the papal contribution to the antislavery movement "was precisely nil before the year 1839" (p. 36).

At the end of the book there are eighteen pages of valuable notes (indexed by author) that display Boxer's usual meticulous care with bibliographical references. Concern with bibliography, however, becomes obtrusive in the rather disappointing chapter entitled "Cultural Interactions," which lists books printed in the missions in foreign languages (pp. 41-45) and missionary writings devoted to foreign cultures (pp. 45-49), but closes with a piquant paragraph on how the Jesuits presented, to Chinese readers, an idealized picture of Europe where there had been neither a Reformation nor religious wars.

A chapter on "Organizational Problems" treats a selection of familiar topics, beginning with internal quarrels involving the regular and secular clergy, in which I would support the conclusion that "on the whole the Jesuits did have higher standards and were often more self-sacrificing than their colleagues in the other orders" (p. 69). A section on frontier missions indicates that these were, in various regions, "the mainstay of colonial rule" and "far cheaper and more effective than large and costly garrisons" (p. 75). Boxer then examines the problems of Padroado and Patronato, and ends the chapter with well-known material on the Inquisition.

The final chapter attempts the difficult task of assessing the long-range impact of Spanish and Portuguese missionary efforts in all areas, from Peru to Japan. Except in the Americas, and perhaps the Philippines, the end result of centuries of effort has been, it seems to me, exiguous. The issue of syncretism needs sharper handling than it gets here. Syncretism often works against Christianity since other religions, such as Hinduism, can incorporate Christian elements without changing in character, while Christianity cannot accommodate religious beliefs from other faiths.

After three centuries "Christian" Goa was still passing laws preventing "Christians" from engaging in pagan practices and four centuries of persecution have not erased African religions from Brazil. Quite the contrary: black Brazilians are

now converting white Christians to pagan Yoruban *candomblé*!

T. BENTLEY DUNCAN
University of Chicago

PETER KARSTEN. *Law, Soldiers, and Combat*. (Contributions in Legal Studies, number 3.) Westport, Conn.: Greenwood Press. 1978. Pp. xviii, 204. \$15.95.

Peter Karsten's serious, even intense, effort to partially unravel and resolve the complicated, troublesome issue of war crimes comes in three phases. The first, a short essay, is designed to show how the "laws of war" evolved. The second, the heart of the book, is an attempt to determine the reasons for noncompliance with these laws. The third, consisting of personal conclusions and recommendations, is designed to reduce the likelihood of future violations.

Karsten divides his reasons for noncompliance into two groups. One consists of "values and attitudes" brought by individuals into the armed forces and is subdivided into "character and personality traits," "ethnocentricity," and "ideological proclivities." The other—factors related to the military itself—is broken down into combat environments, leadership, and the nature of certain weaponry. Within these broad categories, Karsten finds place for the recognized flaws of societies and mankind that lead to war crimes.

Law, Soldiers, and Combat is a step in the right direction, but it falls well short of Karsten's objectives. His approach is overly simplistic, somewhat emotional, and clearly idealistic, whereas his objectives still call for penetrating and balanced excursions into such divergent fields as sociology, psychology, law, power politics, and statecraft, some of which do not easily lend themselves to historical methodology. They also call for comprehensive familiarity with military environments, past and present, both in and out of combat, something rarely attained by even the most experienced soldiers and probably beyond the strictly academic reach. Moreover, Karsten's use of the first person, his often confusing use of examples—a significantly disproportionate number are drawn from the U.S. side of the Vietnam conflict, for example—together with his preference for an outdated translation of Clausewitz and his bed-of-Procrustes approach to such things as the Waffen SS and the term "carpet bombing" tend to weaken his argument and undermine his credibility. Indeed, his overall tone fosters the impression that Karsten is as much the missionary as the scholar, writing to convert as well as to inform.

This is not to say that *Law, Soldiers, and Combat* is

without significant value. Serious studies of this timely and timeless topic inherently deserve serious study. Also, some of his contributions are worthy of special recognition: his emphasis on the responsibility of those up the chain of command who fail to actively prevent such crimes and then cover up the results of their failure; his argument that acts of terrorism which injure the innocent qualify as war crimes; his reinforcement of the argument, oft repeated but seldom heeded, that war crimes are impractical because they are counterproductive; and his compelling emphasis on giving soldiers entering combat a clear understanding of their obligations and options toward potential victims of war crimes.

Taken in perspective, *Law, Soldiers, and Combat* is well worth the reading.

PHILIP M. FLAMMER
Brigham Young University

HEINRICH AUGUST WINKLER. *Revolution, Staat, Faschismus: Zur Revision des Historischen Materialismus*. (Kleine Vandenhoeck-Reihe, number 1440.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 159. DM 13.80.

Revolution, Staat, Faschismus consists of three essays, linked by a common concern with the limitations and usefulness of Marxist categories and insights for historical scholarship. The emphasis throughout is on the limitations of Marxism. In the first two essays, Heinrich August Winkler outlines the views of Karl Marx and Friedrich Engels on revolution and the state and subjects them to a critique based on recent scholarship. In the third essay a similar process is followed for the views of the German New Left on fascism. In each case, Marxism is found to lead to serious errors and misconceptions. Marx and Engels, however, appear as perceptive observers of the developments of their time. Although concerned with construction of a comprehensive theory that would explain seemingly unconnected events, Marx and Engels remained quite flexible in their efforts to understand and interpret events. Winkler finds the New Left, by way of contrast, determined to make fact fit theory.

Of the essays, the third on the New Left theories of fascism is the longest and the most valuable. If it offers more than the first two, it is because it goes beyond an exposition and criticism of New Left theories concerning fascism to offer a detailed, thoughtful consideration of the phenomenon of fascism itself. Winkler provides a convincing analysis of German National Socialism in response to three questions that he considers crucial to an understanding of fascism: (1) what, in particular,

caused certain capitalist societies to turn to fascism while others maintained their liberal-democratic institutions; (2) what was the specific content of fascist policy and by what factors was it determined; and (3) what is the historical place of fascism and its relationship to governmental systems contemporary with it. Because New Left theorists cannot satisfactorily answer any of the above questions, Winkler finds their work largely without merit.

Like most collections of essays, *Revolution, Staat, Faschismus* is only the sum of its parts. The essays address a common topic; each is a thoughtful and enlightening case study. What the book lacks, however, is a summary essay that would develop in more general terms the limitations and the usefulness of Marxism for historians.

Criticisms aside, the book poses important questions in an interesting and lucid manner. Anyone interested in the views of Marx and Engels on revolution and the state or in the problems of defining and explaining fascism will find the essays worth reading. If Winkler ultimately demonstrates the pitfalls of Marxism more effectively than its potential, he nevertheless encourages historians to re-examine the legacy and to take full advantage of the potential.

MICHAEL RICHARDS
Sweet Briar College

ANTHONY JAMES JOES. *Fascism in the Contemporary World: Ideology, Evolution, Resurgence*. Foreword by A. JAMES GREGOR. (Westview Special Studies.) Boulder, Colo.: Westview Press. 1978. Pp. xvi, 238. Cloth \$17.50, paper \$8.50.

The thesis of this book is that fascist regimes are essentially developmental, statist, nationalist, elitist, and authoritarian. Therefore, most Third World military regimes bear strong resemblances to fascist Italy, which becomes the universal model for fascism in the developing world. Underlying this curious vision of universal and omnipresent fascism is the notion that fascism is not a rightist movement but is rather a revolutionary, nationalist, modernizing ideology, far more relevant to the Third World than Marxist ideologies. Anthony James Joes devotes large parts of his rather short work to extraneous and unoriginal attacks on Marxism, as though the Cold War might be won within the covers of his book. (Only rarely these days does one have the pleasure of seeing the term "crypto-communist" used.)

To arrive at his conclusions the author creates an extremely simplistic and tendentious rendition of Italian history and of Mussolini's career in which the Socialist Party is pictured as destroying

any chance of reasonable democratic development by an absurd insistence on revolution in 1919 and 1920. Mussolini emerges as a well intentioned, radical, developmental nationalist who simply could not accept the foolish policies of the Socialists and led a movement of healthy national resurgence, which was allied to but not dominated by the conservative right, until mysteriously in 1938 things began to go wrong. Joes's book verges on the neofascist in its rehabilitation of Mussolini and, although Renzo De Felice's works on Mussolini are mentioned in the bibliography, there is no indication that they were used, much less understood. Nor, for that matter, were non-English sources used in anything but translation, greatly limiting the scope of the research.

The second part of the book traces the similarities between Italy and the Third World. The fact that Italy was culturally quite diverse and infinitely more developed economically does not bother the author, who is determined to bring Italian fascism into the same historical context as Third World militarism. Italy's drive for economic modernization, her incomplete national integration, and the unsuccessful experimentation with Western-style political institutions are seen as the bridge linking the Italian and modern non-Western experiences. Indiscriminately, Nkrumah, Perón, Nasser, the Japanese military (fascist before the term was invented according to Joes), and the Brazilian generals (not Vargas) are brought into this conceptual framework, which the author even feels might be extended to the Soviet Union. For Joes the political spectrum is a circle in which the extremes of right and left are separated by "the thickness of a prison wall."

This is history reduced to anticommunist propaganda. Its general thesis is unconvincing and borders on fascist apologetics, but perhaps its major fault is the failure to distinguish between military and fascist regimes. This failure, coupled with the notion of fascism as a nonrightist movement, so universalizes the concept that it loses all meaning.

ALEXANDER DE GRAND
Roosevelt University

GER VAN ROON. *Europa und die Dritte Welt: Die Geschichte ihrer Beziehungen vom Beginn der Kolonialzeit bis zur Gegenwart*. Translated from Dutch by MARGA BAUMER. (Beck'sche Schwarze Reihe, number 171.) Munich: Verlag C. H. Beck. 1978. Pp. 216. DM 19.80.

To attempt, as Ger van Roon does in this volume, a history of the relationship between Europe and the Third World since the beginning of the colonial period in two hundred pages is an extraordi-

narily difficult undertaking, even though the book is clearly intended for the general reader. Despite several important strengths, van Roon does not entirely succeed.

The author writes with a straightforward, clear style, largely uncluttered by the jargon of social science and Marxist theory. He tries very hard to take a world perspective and to avoid "Eurocentrism." In this, however, lies the source of the book's greatest weakness: its disjointed organization and lack of a real underlying conceptual structure. In renouncing an explanation of imperialism based primarily on developments in Europe in a book about the relationship between Europe and a Third World largely defined by the historical experience of European imperialism, van Roon works himself into a contradiction. His solution is to divide the bulk of his book, after a cursory section on theories of imperialism, into chapters covering Europe's relations with particular areas (Latin America, Africa, and so on). In the discussion of each area, a surprisingly large amount of factual material is loosely distributed around one or two explanatory theories prominent in the literature on the region (for example, dependency theory in Latin America), but neither the regions nor the theories associated with them are related to the other chapters. The book thus becomes a compilation of bits of information on imperialism without much to connect them. Van Roon refers to international economic crises as turning points in imperial history, but he does not build a consistent explanatory structure around them, presumably because it would be too Eurocentric. His use of historical stages to provide organization fails because the stage models are not consistent throughout the book.

The brevity of the book leads, unavoidably, to a tendency to formulate explanations in unqualified terms without supporting or developing them adequately. In most cases, where the explanations are not original, the excellent notes refer the reader to fuller presentations. In the case, however, of van Roon's own very interesting view, advanced in chapter eight, of the relationship between decolonization and European integration, a more complete discussion would have been desirable.

This is, therefore, neither a major scholarly contribution nor a useful textbook, but it is entirely adequate for the general reader interested in the subject.

WOODRUFF D. SMITH
University of Texas,
San Antonio

ANDREW C. HESS. *The Forgotten Frontier: A History of the Sixteenth-Century Ibero-African Frontier*. (Pub-

lications of the Center for Middle Eastern Studies, number 10.) Chicago: University of Chicago Press. 1978. Pp. xiv, 278. \$24.00.

This important book deals with a part of the world and a period of history unfamiliar to most historians except as perceived by Fernand Braudel in his monumental *The Mediterranean and the Mediterranean World in the Age of Philip II*. The author of this volume makes it abundantly clear, moreover, that Braudel's vision of this section of the Mediterranean leaves much to be desired in a number of respects.

Andrew C. Hess begins with four introductory chapters that sketch the nature of North African society as it developed from the fifteenth century until 1520 as a frontier area between a militant, expanding Iberia and a disorganized but resistant Islamic world. His middle chapters are devoted to Ottoman expansion by sea from the east into Tripoli, Tunisia, and Algeria, which unfolded while a more traditional Sa'adian dynasty was revitalizing Morocco in the face of strong Spanish and Portuguese pressures. The sixty-year struggle that ensued in North Africa lasted down to about 1580, when the tempo of war between the Habsburg and Ottoman Empires slackened off. The succeeding half century is dealt with in final chapters that describe how native North African Muslims were able to modify Istanbul's control of their society while continuing to resist Iberian encroachment—creating in the process a unique Islamic civilization of their own. Hess also shows a Spain that was at the same time expelling its last Moriscos and entering the modern world with a civilization opposed to most of what its Maghrebi neighbors and its former Moorish inhabitants held most dear.

Throughout this work Hess emphasizes the distinctiveness of North African culture during this period, which separated its society from that of Christian Iberia and Western Europe in general. Hess also makes it clear that late sixteenth-century Islamic society in both Algeria and Morocco was vital and expansive in many of its cultural manifestations. If one finds his arguments persuasive, as I do, one must avoid being overwhelmed by Braudel's charts, graphs, and statistics, so dear to the *Annales* school. One should, instead, be willing to modify the latter's unitary vision of the sixteenth-century Mediterranean and substitute a vision of two clashing, discordant, and different civilizations emerging on this long-forgotten but vital Ibero-African frontier.

ARCHIBALD R. LEWIS
University of Massachusetts,
Amherst

JAMES W. CORTADA. *Two Nations over Time: Spain and the United States, 1776-1977*. (Contributions in American History, number 74.) Westport, Conn.: Greenwood Press. 1978. Pp. xi, 305. \$22.95.

The subtitle of *Two Nations over Time: Spain and the United States, 1776-1977*, summarizes the content of the book very well. James W. Cortada illustrates in detail the vicissitudes, fluctuations, and variations of the relations Spain was forced to maintain, from the last part of the eighteenth century to the present, with the nation that had emerged as a result of the American Revolution and that would later become "el Coloso del Norte." *Two Nations over Time* fills the vacuum that previous studies had left concerning the political and diplomatic relations between Spain and the United States during the last part of the eighteenth century and throughout all of the nineteenth century.

Since the beginning of the American Revolution Spain had become a great barrier in the territorial expansion of the United States that did not end until October 1898, although "by 1820 the United States had more than doubled the size of its territory, and mostly at Spain's expense" (p. 19).

Cortada's book consists of a brief preface, fourteen chapters, a conclusion, two appendixes ("Spanish Envoys to the United States" and "American Envoys to Spain"), and an extensive bibliographic essay (pp. 284-98). Although *Two Nations over Time* is "a summary, it is also directed to scholars who may want an introduction to the subject" (p. ix).

Cortada's work is meticulous, dense, motley, and sometimes prolix, because, in his eagerness to present each chapter as an independent unit, the author sometimes repeats what he has already stated. On the whole, however, Cortada's book is an exhaustive and excellent piece of work. The gratitude expressed to various professors, the two chapters dedicated to cultural relations (chap. 8, "A Century of Cultural Relations," and chap. 14, "Recent Cultural Relations"), and the two appendixes previously mentioned cause one to believe that this work was originally presented as a doctoral dissertation.

Even though, in the preface, the author thankfully mentions the collaboration given by the staff of the Archivo Histórico Nacional and of the Biblioteca Nacional de Madrid, he does not seem to have dealt with original documents. His references are mainly based on studies or opinions of other authors. He cites original documents, in English as well as in Spanish, from the beginning of chapter twelve, when he studies the relations between Spain and the United States at the start of the Spanish Civil War.

Perhaps because of this lack of original sources, the exposition of the Spanish contribution to the independence of the United States does not seem totally objective; Cortada repeats more or less what has been said by other American historians.

Nevertheless, Cortada is to be congratulated. *Two Nations over Time* is a fundamentally important contribution to the history of Spanish-American relations.

ENRIQUE FERNÁNDEZ Y FERNÁNDEZ
Eastern College,
Saint Davids, Pennsylvania

WILLIAM ROGER LOUIS. *Imperialism at Bay: The United States and the Decolonization of the British Empire, 1941-1945*. New York: Oxford University Press, 1978. Pp. xiv, 594. \$19.95.

Anglo-American relations in the twentieth century have more often been veiled than examined because of a mutual suspicion that nakedness would prove embarrassing. William Roger Louis's book—wrongly subtitled, since it deals not with decolonization but with the undermining of the British imperial structure—confirms how right people have been to think so. World War II confronted "the cousins" (as the Americans are still jocularly classified among British bureaucrats) with ideological dilemma as well as with military conflict. One inescapable fact of international life was their alliance with the British empire, escape from which was the necessary prelude to their own national existence and dignity. They had to put their systems suddenly into reverse. They did not enjoy the experience.

Churchill and his men were determined that the empire should survive: otherwise, why fight at all? Roosevelt and his advisers hoped that it would not. The American hope prevailed over the British determination, for the United States is still around and the British empire is not. In this fine and fascinating book, Louis examines the wartime archives to discover how this development came about.

The outcome surprised everyone. Neither the Americans nor anyone else, Louis rightly insists, foresaw the speed of imperial downfall. But the themes of the Atlantic Charter (August 1941) and their widespread acceptance spotlighted the entire colonial issue. The British colonial office, pressed to justify itself, found that its previous policy declarations were, as Harold Macmillan himself admitted, "scrappy, obscure, and jejune." Although British officialdom continued to think that Roosevelt's views were half-baked—as Louis agrees (p. 150)—by 1942 Washington had decided that the

age of imperialism was ended and had projected the idea of a timetable for its dissolution. The gloomy complaints of British Ambassador Halifax concerning "those Jeffersonian forms so dear to the American heart" and his quite accurate observation, with which Louis concurs, that the British tend to proceed from the particular to the general, "but the Americans do just the opposite," were beside any point that mattered. Gladwyn Jebb from the British Foreign Office predicted that the American stance would encourage the nefarious notion that the hope for the future lay in a great multiplication of small national sovereignties. The British official view was, and remained, that independence was a political catchword that had no real meaning apart from economics. Was it really intended that every island in the West Indies and in such unviable places as the Seychelles should be independent?

Yes it was. Roosevelt's trip to Casablanca, routed via Britain's colony of Gambia, hardened the president's views—"fifty cents a day for labor, the most horrible thing I have ever seen in my life!" He was convinced that the colonial system bred war (p. 225). At Cairo in December 1943, the Foreign Office prepared a case thoroughly to put before FDR, but, remarks Louis, "unfortunately for the British, the President preferred to consult the Chinese" (p. 278). Meanwhile, the U.S. Joint Chiefs of Staff incubated cheerfully imperialist views in regard to the future of the Pacific and by July 1944 concluded that the British empire had already ceased to exist as a land power (p. 377).

The continuing American insistence that colonies should be put under trusteeship as a preliminary to independence forced the British to produce, in December 1944, a state paper entitled "International Aspects of Colonial Policy." They had not considered this issue in happy, prewar days. Churchill did not read the paper; he informed Anthony Eden that "Hands off the British Empire is our maxim, and it must not be weakened or smirched to please sobstuff merchants at home or foreigners of any hue" (p. 433). But no Colonial Office man accompanied him to Yalta in February 1945, where he found himself "put in the dock and asked to justify our right to live in a world we have tried to save" (p. 458). It was at Yalta, as Louis stresses, that the Trusteeship Council of the United Nations was conceived.

Louis's book, as full of wit and insight as it is of information, will instruct every modern historian. It needs a sequel, as 1945 is no proper terminus for this strange story. May the author soon provide one.

A. P. THORNTON
University of Toronto

THOMAS B. LARSON. *Soviet-American Rivalry*. New York: W. W. Norton. 1978. Pp. xii, 308. \$13.95.

As the Carter administration seeks haltingly to stabilize its own version of detente with Moscow, a cool look at the strategic context of Soviet-American competition since World War II can place Washington's current concerns in a salutary perspective. In surveying this context, Thomas B. Larson is determined to shed his "national skin" and examine the rivalry dispassionately. He largely succeeds.

His level-headed treatment of the Soviet and American political systems avoids the usual pitfalls of popular, and much academic, discourse, but he rightly stresses the primacy of the economic order. In assessing the current stage of the Soviet economy, he often counters prevailing opinion. The enormous lag in Soviet agriculture, he insists, cannot be attributed mainly to its collectivist nature, and he emphasizes the substantial improvement in the standard of living and the narrowing of income differentials since Stalin's death, factors too often neglected in assessments of Soviet stability and the degree of its popular support.

Military competition is less important in the long run than economic rivalry. Larson's analysis of neither the strategic arms race nor the recent rapid growth of conventional forces will provide any sustenance to alarmists. Since neither side can win an arms race, the military competition will in effect be stymied.

More broadly controversial is the author's overall economic projection: the Soviet economy will continue to grow more rapidly than the American and by the end of the century achieve a gross product comparable to the American. Other futurologists will disagree, but the assertion itself should inspire to a second and third look those analysts and propagandists who constantly harp on the failures and intrinsic weaknesses of the Soviet economy and take a most benign view of America's economic future.

The author is most refreshingly sane in his treatment of the ideological conflict, an area in which the roots of cultural bias are deepest and the afterglow of the Cold War most luminous. The semantics of "freedom" and "democracy" on both sides of the ideological divide is given even-handed treatment, and the struggle between "totalitarianism" and "imperialism" is fairly put as a contest of epithets more surcharged with feeling than meaning. He pauses to note the anachronistic retention of "totalitarian" and "terror" in the American right-wing vocabulary as a useful tool for separating Moscow from the dictatorships Washington finds it expedient to support.

Clear as he is here, I find some mild semantic

confusion in his final chapter on Soviet aims and prospects. Lower-case "communism" (as in "sweep toward communism" or "the link between communism and . . . underdevelopment") appears to mean one thing, upper-case Communism another ("Communist successes," "Communist appeal," "Communist potential"). Such phrases as "Marxist-Leninist rule" and "Communist or quasi-Communist rule" are even more confusing.

In a survey of this scope some aspects of the competition must be given cursory treatment. Larson devotes only a few paragraphs each to Moscow's support of national liberation movements, to scientific research, and to intelligence and political action operations. These deserve further analysis by "the man from Mars" approach, as do the institutional mechanisms that Washington and Moscow have at their disposal for taking action abroad.

HARRY ROSITZKE
Middleburg, Virginia

ALEX ROLAND. *Underwater Warfare in the Age of Sail*. Bloomington: Indiana University Press. 1978. Pp. xvii, 237. \$16.50.

Military and naval history are often attacked by slabminded historians possessed of a zealous and narrow sense of propriety. Too often these historians lump scholarly research on systematic blood-letting under the pejorative of "warmongering" and dismiss it out of hand. Alex Roland's work defies the pious hypocrisy of these members of our profession. To be sure, Roland deals with war and death, but his is a history of ideas—ideas of the mine and the submersible. It is also more. It is a history of technics, a treatise on human nature, and a respectable piece of analysis. This is reflected in the author's statement that "underwater warfare . . . mirrored almost perfectly the rapid transformations of science, technics, law and morality in the three centuries of its gestation" (p. 4).

Cornelius Drebbel, Robert Boyle, Denis Papin, David Bushnell, Robert Fulton, Samuel Colt, and Matthew Fortaine Maury are the "Davids" whose achievements Roland selected to examine. He has given us a balanced view of these men, their problems, and their creations and a minor disappointment in ignoring the work of Johnson, Shuldham, Cervo, and Villero.

In dealing with Drebbel, Roland shows how early technics, science, and magic were poorly defined in the eyes of authority. He explains the moral conflict within kings over the "weapons of the Devil" and the ignorance of royalty's military advisors. This poor definition, conflict, and ignorance prevented many of Roland's inventors from

perfecting their ideas. Two important attitudes of the Enlightenment are treated in this work. One was Europe's rediscovery of war as an instrument of policy. The other was the decline of the concept of weapons of the Devil. Neither was the fruit of military minds.

Robert Fulton's prescription for the military, his genius, and his failings are evaluated. Fulton understood that the military had to do two things to make underwater warfare effective: accept the mode of fighting; and train in the use of the mode. "Systematize," Roland says, was Fulton's unflagging martial advice. When the military accepted this urging, underwater warfare became effective in battle. In a brief and notable paragraph Roland evaluates Fulton, Samuel Colt, and their experiences in promoting this way of war. Both, he claims, were secretive, tactless, and boastful. Hostile military authorities, rotating functionaries in government office, and shoddy treatment by both impeded the two inventors. The brutal realities of the Civil War eliminated this conflict between the inventors and government and introduced effective subsurface war.

In his treatment of the evidence Roland is bold, is unafraid of making educated guesses, and evaluates his sources. He displays his judgment in examining the sketchy record of David Bushnell's sojourn in France after the American Revolution. Too, he suggests that Matthew F. Maury had more complete knowledge of this mode of fighting than his papers suggest. This is a reasonable surmise to one familiar with the contemporary evidence.

In his conclusion Roland opts for the principle of continuous evolution rather than independent invention as the source and sustenance of this mode of combat. He also sets out a list of the objections military men posed to this style of fighting. This little passage should be read by those who doubt the value of thinking to the military.

This is a survey of the important progenitors of submarine warfare and an explanation of their efforts as intellectual and technological activities. Despite his grating insistence on referring to these men as "Davids" and a narrow selection of graphics, Roland has produced a valuable work. He has a broad view of his subject, command of a formidable vocabulary, and scholarly audacity. He has given us a sound piece of scholarship illuminated by clear perceptions and a trace of brilliance. For scholars of history, ideas, and technology this work has merit. It should be read by naval officers and government functionaries. It reflects decided promise on the author's part.

LAWRENCE C. ALLIN
University of Maine,
Orono

ROBERT P. MÜLTHAUF. *Neptune's Gift: A History of Common Salt*. (Johns Hopkins Studies in the History of Technology, New Series, number 2.) Baltimore: Johns Hopkins University Press. 1978. Pp. xviii, 325. \$22.50.

To find an uncommon history of a common thing is rare. But such has been this reader's experience with *Neptune's Gift: A History of Common Salt*. Robert P. Multhau has succeeded in making manifest the varied and protean aspects of his mundane subject matter succinctly and authoritatively.

With such a subject matter, it would have been possible for the author to discipline the reader with a blow-by-blow description of interminable variations on how to extract salt from natural formations; or to immolate the reader with the infinitely many ways that the monographic zealot devises to describe the minutiae of the relations among a taxable staple and government finances; or to bury the reader with the fine particles of a dusty antiquarianism. The author elected to narrate the story from a number of viewpoints, however, each of which has its own validity, and each of which "carries a particular theme from the distant to the immediate past in a comprehensive way" (p. xvi). That the reader perceives a unity among these several viewpoints is because the author has shown a judicious care (and what most certainly must have been a fair amount of painful ruthlessness) in pruning away all matter but that which is not only relevant but immediate to the viewpoint in hand. (Just how much must have been pruned away is suggested by the thirty-four pages of bibliography and the many credits to libraries both here and in Europe.) That these several viewpoints of the author should overlap, not with repetition but with successive illumination, is evidence of the care shown in writing the book.

The book is divided into two major divisions—the early history of salt, when its main importance was as an item to be taxed by someone and as an item in the diet of the consumer, and the more recent history, when salt is treated as a chemical and is conceived and applied to society's needs within the matrix of a scientific theory.

In the first division, salt production was an art and a craft, carried on by an agricultural society; it was an activity that produced trade and taxes and that created institutions of its own within that society. By the seventeenth century, these institutions were losing their local features and were acquiring the characteristic features they needed to play a role in the state that sustained them. By that time, saltmaking had gone beyond the stage of protoindustrialization, in which there was a mere multiplication of productive units, and was passing into the stage where the major problem was

that of increasing the efficiency of the operation, with the appearance of special artisans, men like the *Feuerkünstlern*, who sought to increase the output, while keeping the energy costs down. The approaching shadow of an energy crisis, the shortage of the basic fuel (wood), brought this first phase of salt production to a close.

The second major division of the book deals with the age of salt as a chemical, when one turns to deep drilling, when many of the problems of using the relatively new fuel (coal) have been worked out, when the newer problems are the geological ones of where to find salt, and the chemical ones of how to obtain "good" rather than "bad" salt. As one enters the age of science one also acquires the ability to "transform" salt into other, and even new, chemicals; one is no longer at the beck and call of nature but increasingly can command nature so as to "create" a multiplicity of other, new chemicals from salt, one of the most primitive and readily available of all materials. The author explores, and with ease, the current, ever-shifting relationship between salt production and that of its various progeny, some of which succeed in surpassing their source in importance. As the author dryly points out in his final section, "Reflections," all one needs for these transformations is energy. In this second division, the author elucidates with easy authority and simple clarity how salt, in its natural production and its scientific transformations, has generated many scientific theories. Such theories have become the guides to the further uses of this most common of all chemicals.

Neptune's Gift is an excellent work, of importance to the generalist as well as to the specialist, and is deserving of a wide audience. The excellence carries into the details of the book—its many choice and rare illustrations, the full index that explicitly indicates the key passages, and the statistics that, without any pretensions at completeness or undue accuracy, provide the historian with ever-welcome indications of the significance of this universal substance and of the industry associated with it.

W. JAMES KING
Texas A & M University

CHARLES W. JONES. *Saint Nicholas of Myra, Bari, and Manhattan: Biography of a Legend*. Chicago: University of Chicago Press. 1978. Pp. ix, 558. \$28.00.

In this book, the culmination of decades of interest and scholarly investigation, Charles W. Jones has set for himself an enormous task. N, as Jones calls his subject to distinguish between Bishop Nicholas of Myra and the legend that grew up about him,

was both an exceptionally popular saint and one whose popularity was extraordinarily long lived. Thus, the materials of this study extend from the first mention of a church dedicated to N (Constantinople, 555) to his deletion from the new calendar of saints approved by Pope Paul VI (February 14, 1969), from the earliest anonymous Greek record of an N miracle to Clement Moore's *A Visit from St. Nicholas*, from Byzantine icons to the cartoons of Thomas Nast, from the apparently captivating melodies of the tenth-century N liturgy to *Santa Claus Is Coming to Town*.

To bring order to this mass of materials and to relate these developing manifestations of N's cult to broader historical changes, Jones has cast his work in the form of a biography, the "biography of a legend." Adapting Shakespeare, he argues that legends, like men, have their seven ages, and he traces the life of N from his mute infancy (the period before his personality found verbal expression) in Myra and Constantinople, through boyhood, adolescence, knighthood, maturity, and old age, to his senile second childhood in New York City. Not only does Jones see N's personality changing with the passage of time, but he sees the ages of the legend corresponding in nature as well as in number to the ages of man. Thus, the adolescent N was "more fractious" than he had been in his puerile years (p. 119), and the mature N was a round-bellied member of the late medieval establishment. In Jones's interpretation, not only do the ages of man become ages of the legend, but the ages of the legend become epochs of Western culture as well. N's infancy, for example, corresponded to the nascence of Christian orthodoxy, his knighthood to the Crusades, and his old age to the Reformation. Jones brushes aside objections to this explicitly organic interpretation of a historical process, and the living quality of a legend offers some justification for his approach. Yet he allows his Shakespearean pattern to assume a normative rather than a merely illustrative function, and the readers who accept these ages as stages of both N and Western culture will probably also be those who agree with his conclusion that not only has history governed N, but indeed "N has governed history" (p. 369).

After a lifetime as a respected and, in the most positive sense of the word, a "conventional" scholar, Charles Jones has written a most unconventional book. Filled with literary and linguistic learning, it is a bold and very personal statement of his views regarding history and civilization and N's place in their development. As such, it will find both devoted admirers and determined detractors.

JOHN M. MCCULLOCH
Kansas State University

ANCIENT

S. C. HUMPHREYS. *Anthropology and the Greeks*. (International Library of Anthropology.) Boston: Routledge and Kegan Paul. 1978. Pp. xi, 357. \$20.00.

This book collects previously published articles and includes both an introduction providing an overview and some additional notes providing updating or revision. The arrangement is not random or chronological but articulates the pieces according to complementary themes (classics and anthropology, economy and society, and societal structure) and elucidates the author's own development. Not all of the works to date are included (for example, the essay on bastards in the *Journal of Hellenic Studies* [1974] is missing), although a collaborative effort by Momigliano on religious tolerance and free speech is. And of course work continues—the article “Public and Private Interests in Classical Athens” in *Classical Journal* [1977/78] clearly belongs to the present milieu.

The book thus allows viewing whole the developing ideas of a scholar who has so often insisted on studying societies whole and whose favorite words include “holistic,” “system,” and “structure.” S. C. Humphrey's attempt to understand changing Greek (mostly Athenian) society by focusing on its social institutions and the contexts of communication they permitted and the attitudes they reveal discloses much about her own methods of analysis, her own *mentalité*, and her own efforts to transcend the boundaries between disciplines. She herself becomes a valuable model for classicists who frequently think their interpretations are free from cultural and intellectual bias. But, as the essays, particularly those on Polanyi and Gernet, show, she tends to regress to a scholarly introversion in which she becomes fascinated with intellectual theories per se and generates opinions on scholars' opinions about other scholars. One wonders at times whatever happened to the actual data from antiquity.

The interpretative models used here produce a mixed return. Paradoxically, abstraction supposedly facilitates the highlighting of individual peculiarities, although it entails some evident disregard for field studies of individual sites or cultures. The absence of names like Pitt-Rivers, Peristiany, Campbell, and Bourdieu from the forty-two-page bibliography is symptomatic. Shrewd observations on ports of trade, on the functional flexibility of the Athenian “bank,” on kinship optionality and the limits of the prosopographical method for Greek history, and on Ringo tee-shirts, *kalos* vases, and the pottery trade coexist with an analysis of transcendence and social structure that often dismays

because it frequently forces data into patterns that do not do them individual justice. What Greek word, for example, covers the spectrum given the word “intellectual”? The author may herself have committed the modernist fallacy. Indeed, her description of the intellectuals working to develop a new style of communication, general theories, and status-free discourse while standing outside the political arena makes them seem rather like Humphreys. But could not Pericles, friend of Anaxagoras and Pheidias (and even pointy-headed!), be considered an “intellectual” though he was hardly on the outside?

Handling of poetic texts needs rethinking, and her treatment of Attic drama sets up categories that are too neat. Saying that drama was outside the scope of the democracy's political institutions implies an anachronistic definition of politics in classical Athens. Statements like “drama left less to the imagination of the audience than earlier monodic or choral poetry” (p. 230) do not encourage confidence in the model fostering them. Her accurate perception of kinship's importance in drama conflicts with her claim for an impersonal style as dominant in public life: justifiable arguments for the tension between private and public need not diminish the force of personality and family in “politics.” Pericles may have been frank in more ways than one when he called his Athens a tyranny. Claims for growing political secularization must still accommodate the gods' presence in the very oaths and inscriptions that sealed and codified public actions.

It is not surprising to find the author finally searching for yet more variable and individual-aware models. But whether the models are convincing or not, the fresh approach of each piece here reveals innovative thinking; and Humphreys's proposals for archeological method, classical curricula, and their integration with comparative anthropological outlooks provide an important stimulus for keeping the Classics ever new.

W. E. HIGGINS
Center for Hellenic Studies,
Washington, D. C.

DONALD W. ENGELS. *Alexander the Great and the Logistics of the Macedonian Army*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xiv, 194. \$15.00.

Historians have long been aware that the strategy and tactics of Alexander the Great were in large part dictated by the availability of supplies along his route. Given the scanty nature of the ancient evidence on the topic, it is hardly surprising that

until now there has been no comprehensive study of the logistics of Alexander's army. Donald W. Engels displays a keen appreciation of the limitations of land transport in antiquity and the subsistence nature of ancient agriculture. By drawing extensively on modern geographical, archeological, and paleo-ecological research as well as accounts of early travelers, he has made skillful use of the limited ancient evidence.

It is, of course, impossible to determine precisely the number of troops, camp followers, and animals that accompanied Alexander on his trek, but Engels overcomes this problem by establishing a consumption rate as his starting point. This is the weight of food and water consumed per individual man and animal per day, based on the known nutritional requirements for each. He then determines a carrying capability by estimating the weight that an individual man or animal could carry. It is the ratio between the consumption rate and the carrying capability that is the crucial element in this study. This ratio is expressed by a mathematical formula that permits adjustment for numbers, terrain, the availability of food and water, and other variables. Mathematics leads to the conclusion that any army solely dependent on men and animals for transportation cannot, no matter what its size, carry more than a four day supply of both food and water. By the fifth day they would have consumed all the supplies they had brought with them. These figures place Alexander's problem in sharp focus.

With this as a starting point and concentrating on areas where logistics were of concern, Engels traces Alexander's path from Macedonia to the Hydaspes and back to Babylon. He has fresh insight to offer on Alexander's route, especially in the east, but his primary considerations are to determine how Alexander secured provisions and to show how the problem of supplying his forces affected the timing and direction of his movements. He concludes that wherever possible Alexander sought to secure provisions through the advance surrender of local officials. Where this proved impossible, various other means were employed, including taking hostages, establishing garrisons, dividing his army into smaller units, and especially securing intelligence on the area to be traversed. In a word, every movement of Alexander's army, even the near disastrous crossing of the Gedrosian desert, was carefully planned with the availability of supplies uppermost in his mind.

The author recognizes that much of what he has to say is hypothetical, and there are indeed many things with which one might take issue. Yet this should not obscure the fact that the method is sound and the research thorough. There are five appendixes and sixteen original maps. The proof-

reading has not been particularly careful, but otherwise this is an impressive doctoral dissertation.

THOMAS KELLY
University of Minnesota

HINNERK BRUHNS. *Caesar und die römische Oberschicht in den Jahren 49-44 v. Chr.: Untersuchungen zur Herrschaftsetablierung im Bürgerkrieg*. (Hypomnemata, number 53.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 199. DM 38.

There is surely no more important task for the political historian than investigation of the origins and support of political power, particularly overweening political power, such as that which leads to the downfall of previously existing institutions. Hinnerk Bruhns has done this with considerable success for the last years of Caesar's career. In doing so he has not so much upset the generalizations of his predecessors (notably Sir Ronald Syme and Shackleton Bailey) as modified them. The results seem fairly persuasive and probably will require another study as detailed as this to shake.

The method is primarily prosopographical. Bruhns examines the evidence for the attitude of each member of the ruling class, mostly senators of course, toward Caesar and the prospect of joining him or Pompey, or remaining neutral, for a time or throughout. The results are fairly impressive for leading members of the nobility, those who counted. Apart, however, from the incomplete documentation, one is a bit disturbed by Bruhns's use of Cicero's testimony, not as evidence about facts or the attitudes of individuals but as a means of checking the attitudes of groups against the propaganda of Caesar (and his opponents). Cicero was not only a single individual, and as a *novus homo* not really typical of the ruling class, but a man notoriously given to wishful thinking. One supposes that no historian of modern times would accept so slender an authority by itself. Naturally, however, this is not only the usual making of bricks without straw, necessarily characteristic of the work of historians of antiquity; it is part of the epistemological problem, posed to all historians, of trying to determine objective truth about attitudes from biased testimony. At the least, Bruhns's detailed exposition and careful consideration of the evidence that we do have do much for his case.

Very few members of the upper classes trusted Caesar, as Strasburger has shown in detail and Bruhns confirms. The really die-hard opponents of Caesar at the beginning of the civil war were a small minority of the Senate. It is usually held that exiled persons *en bloc* supported Caesar; the reality was more complex as Bruhns shows. Caesar's

"party" did not exist as such; throughout, his adherents were those who for widely different personal reasons sought fame and fortune (and *auctoritas*) on his side. It is no surprise that high-ranking senators tended to prefer Pompey to Caesar; low-rankers frequently favored the latter. And an interesting proportion of the consulars (those who had attained the top and had less to gain or lose) were neutral. "All Italy" went over to Caesar, not in a rush at the beginning, but as the progress of the war foretold his victory. And perhaps the most novel and important demonstration by Bruhns is that political realities in fact considerably limited the power of Caesar as dictator even to his death.

STEWART IRVIN OOST
University of Chicago

GRAHAM WEBSTER. *Boudica: The British Revolt Against Rome, AD 60*. Totowa, N.J.: Rowman and Littlefield. 1978. Pp. 152. \$12.50.

Graham Webster already has to his credit two works on the early years of the Roman conquest of Britain, both in collaboration with D. R. Dudley: *The Rebellion of Boudicca* (1962) and *The Roman Conquest of Britain* (1965). The chief interest of this new study lies in the updating of the archeological evidence, on which Webster writes with the authority of a distinguished excavator who has made the lowland zone his special territory.

On the archeological evidence prior to A.D. 43, which includes some tricky numismatics, Webster is clear and judicious. He is also an excellent guide to the Claudian-Neronian sites, like The Lunt, Camulodunum, and Verulamium. His generosity to other scholars and his own passionate commitment to the cause of archeology shine through. So does his sense of humor. There is also a lot about how the Roman army worked, on which Webster is an authority.

The book seems to be addressed primarily to the British public, brought up on a romantic view of the queen whom most Britons would still call Boadicea. The statuary group from Civic Hall, Cardiff (plate 1), showing her and her two nubile daughters, may be compared with similar superbly overdignified portrayals of Vercingetorix discussed by André Stell, *Astérix, l'épopée burlesque de la France* (1978).

Where archeology does not help, and Tacitus and Dio are too brief, Webster speculates freely. The lay reader must be alert to distinguish what "did" happen from what Webster says "would have" or "must have" happened. Nowhere is this more so than in the account of Paullinus's reaction to the uprising. Even if he did take a galley from Anglesey to Chester, need one speculate on the

direction of the wind? His motive for getting to London was surely more than just "the urgent need to see for himself the extent and state of the revolt" (p. 93). Poenius Postumus, temporarily commanding the legion at Exeter, three days' march closer to London than Paullinus's own legions, disobeyed orders to join him. Webster makes excuses for him. What sort of signal network did the Romans have? Webster does not raise the question, though elsewhere he was not unaware of it ("The Claudian Frontiers in Britain," in *Studien zu den Militärgrenzen Roms*, 2 [1971]: 42-53).

Some misprints may mislead: Broxtowe, not Broxstow (p. 18); the definition of "duodecimal" (p. 25); *fluvius*, not *fluxius* (p. 59); Thracian revolt 11 B.C., not A.D. (p. 129); *segmentata*, not *segmenta* (p. 142). For Fergus Millar's doubts about the traditional view of *municipia* (pp. 96, 123, 142), see *The Emperor in the Roman World* ([1977], pp. 398-406). I have argued that cavalry tombstones (p. 118) are not evidence for the size of horses (in *Akten des XI. Internationalen Limeskongresses* [1978], pp. 659-65). Finally, the index is inconsistent and misleading. The most fatuous entry is "Cole, Old King."

C. M. WELLS
University of Ottawa.

PHILIP ROUSSEAU. *Ascetics, Authority, and the Church: In the Age of Jerome and Cassian*. (Oxford Historical Monographs.) New York: Oxford University Press. 1978. Pp. x, 277. \$23.50.

Philip Rousseau's study carries us from the primitive desert fathers of the fourth Christian century onward via monasticism and Athanasius and Jerome to the West and Martin of Tours and John Cassian. All were ascetics; nearly all were powerful and confident. Their ideas about life changed from emphasis on eremitic solitude to the expression of ambition for control of the more ordinary life of the church. Without overtly using psychological categories, the author lays emphasis on their spiritual authority as their movement penetrated Gaul. He says he is writing "an essay in interpretation, rather than narrative history." Along the way he provides many insights into the rise and significance of asceticism in ancient Christianity in East and West alike.

At the same time, the fact that the book was "in some ways written backwards" and its "steps sideways" construction make it difficult to follow. In addition, there seems to be a tendency to overinterpret ancient texts, notably in regard to Jerome. One cannot apply all of his description of the little girl Pacatula to himself when it includes the words "ignorant of the past." It is wrong to

call him "always . . . an urban guerrilla at heart" (p. 123). J. N. D. Kelly did not miss the point of Jerome's exegesis in *Ep.* 52, 2, for the *gentilis litteratura* is not identified with David (p. 127). Jerome, like other Christian exegetes, was simply citing both pagan literature and scripture. There is no "telling ambiguity" in *Ep.* 125, 2 (p. 132). The point in a passage in Acts "to which Jerome alludes" is not Jerome's point, and the complete obscurity of the last two sentences on this page is due to the mistranslation of *quid venenatorum animalium desertum huius saeculi nutriet* (the kinds of poisonous animals the desert of this world feeds) as "the kinds of animal that one may hunt for, as food in the desert of this world."

Earlier periods of church history provide materials that Rousseau's themes can illuminate. Thus the problems presented to bishops by ascetic Montanists and Encratites were like those presented by hermits and monks. So was the difficulty that arose between the ascetic teacher Origen and the bishop of Alexandria. Above all, there is the strange story of the life of the ascetic Narcissus, bishop of Jerusalem at the end of the second century. His activities (miracles, exile to the desert, restoration) have been played down by Eusebius but remain significant. Rousseau notes topics for continuing his own work into the future; here is one for the past.

ROBERT M. GRANT
University of Chicago

MEDIEVAL

BARBARA W. TUCHMAN. *A Distant Mirror: The Calamitous 14th Century*. New York: Alfred A. Knopf. 1978. Pp. xx, 677. \$15.95.

A Distant Mirror is a book about the Middle Ages, more particularly about the fourteenth century, which was written by a specialist in modern history and intended for a popular audience. How does a specialist in medieval history review such a work for a journal read essentially by professional historians? Certainly, it would not be appropriate to devote the several hundred words allotted to this review to a truncated scholarly critique that made it abundantly clear that the *apparatus criticus* is appallingly inadequate, that the bibliography is riddled with significant *lacunae*, and that, for a reputable modern historian who enthusiastically blundered into the fourteenth century, Barbara W. Tuchman has failed to gain a firm grasp of the world through which she rambles. To medievalists, suffice it to say, nothing of value—either new

data or useful interpretations—has emerged from Tuchman's seven years of research.

What then of the popular reader for whom this book is intended and whose knowledge of the medieval world generally comes from some combination of exposure to Sunday school, movies, television, Sir Walter Scott, and high school social studies classes? Those who just love the Middle Ages may well find that *A Distant Mirror* provides the kind of gossip that they will not get in a college-level medieval history course. Indeed, Tuchman has provided a readable fourteenth-century version of the Fuzz n' Wuz (cops and corpses) that dominates the evening news on television.

Tuchman's emphasis on plunder, rape, burning, carnage, and high-level stupidity (not necessarily in that order) is balanced by her preoccupation with lying, cheating, deceit, immorality, and high-level insanity (not necessarily in that order). All this and more is provided—in lurid detail when she can find it—and frequently it is presented in quotations from archaic English translations of Old French and Latin chronicles. Tuchman has scoured the published and translated literary and narrative sources—documents have generally been neglected—in her search for scandal and has used their "evidence" with more credulity than critical attention.

Tuchman would have us believe that the fourteenth century is *A Distant Mirror* of our world today. From this work, however, emerges neither a panorama of the fourteenth century nor a biography of Enguerrand VII de Coucy, Tuchman's "hero" (enough data survives concerning de Coucy to write a useful monograph), but a far off reflection of what Tuchman seems to see as central or, at least, marketable about the world in which she lives. Television "news"—high in the ratings but low in thought and balance—seems to have shaped her view. In response, she apparently has selected her data from the fourteenth century to conform to this distorted picture of the modern world. Whether Tuchman set out consciously to shape the fourteenth century in the image of the twentieth, as she sees it, is unlikely. More probably, it is the result of her relative ignorance of medieval history combined with an unerring sense of what will be popular.

Readers of Tuchman's earlier works will be particularly disappointed to learn that her generalizations about medieval warfare are grossly inaccurate. Her discussions of individual psychology and group psychology are equally foolish. She seems to have little understanding of what motivated the people about whom she writes and generally resorts to clichés such as chivalry or individual neuroses as explanations. *A Distant Mirror* makes clear by what it is not that the American

reading public deserves access to history that holds a middle ground between the unreadable monograph and unreliable gossip.

BERNARD S. BACHRACH
University of Minnesota,
Twin Cities

TILMAN STRUVE. *Die Entwicklung der organologischen Staatsauffassung im Mittelalter*. (Monographien zur Geschichte des Mittelalters, number 16.) Stuttgart: Anton Hiersemann. 1978. Pp. viii, 349. DM 154.

The series *Monographien zur Geschichte des Mittelalters* has produced a number of important historical studies by Prinz, Bosl, Kienast, and Sprandel, most of them dealing originally and very competently with social history and the social history of ideas. Tilman Struve's book at first glance appears to deal instead with the old chestnuts of medieval political theory and with the most fatigued of its metaphors, that of the body politic. But Struve knows as well as any that behind the medieval compulsion to write and think in metaphors there lay a profound desire to establish principles of social order and to describe them analytically. Struve begins with an analysis of Otto von Guericke's limited legalistic concern with the state as organism and then works toward a survey of the *mentalité* of medieval thought, evidently (see the note on his forthcoming work, p. 5, n. 8) moving toward an anthropology of medieval thought. At the base of this thought Struve discerns a powerful sense on the part of medieval political thinkers that particular human associations were in essence parts of a cosmological *Ordnung* and that their expression of this awareness changed as specific historical structures changed.

The introduction to the book reviews previous scholarship thoroughly. The second of the work's eight sections introduces the beginning of the metaphor of the state as organism in Greco-Roman culture and ends with a discussion of Seneca's *De clementia*. The third section treats the "bridges to the Middle Ages," the ecclesiological tradition of the metaphor in St. Augustine and the cosmological tradition in Chalcidius's commentary on Plato's *Timaeus*, and the ideal state described in the work of Al Fārābī. The fourth section takes up the theme of the state as a function of the *corpus Christi* and the influence of the struggle between *regnum* and *sacerdotium* during the Investiture Conflict, ending with a short account of the revival of cosmological ideas in the twelfth century and their relation to political thought. The fifth section deals with what is probably the *locus classicus* of the theory of the state as organic, the work of John of

Salisbury. The sixth section deals with the influence of the revival of Aristotle in the work of Aquinas, Ptolemy of Lucca, Aegidius Romanus, Engelbert of Admont, and Dante. The seventh and eighth sections treat the superstars of thirteenth- and fourteenth-century political theory, Ptolemy of Lucca, Aegidius Romanus, John of Paris, and Marsiglio of Padua. The ninth section summarizes the book's argument and constitutes a summary of the importance of the theory of the body politic in the understanding of the medieval conception of the political community. There is an appendix of Aristotle citations and a thorough and up-to-date bibliography. The book is handsomely and very competently produced, as one familiar with the series might expect.

EDWARD PETERS
University of Pennsylvania

WOLFGANG VON STROMER. *Die Gründung der Baumwollindustrie in Mitteleuropa: Wirtschaftspolitik im Spätmittelalter*. (Monographien zur Geschichte des Mittelalters, number 17.) Stuttgart: Anton Hiersemann. 1978. Pp. x, 235. DM 98.

Wolfgang von Stromer has written an essay on the origin of the upper German manufacture of fustian (*Barchent*, a cloth made of linen and cotton mixed) that challenges a number of comfortable orthodoxies about late medieval economic life. Economic historians are used to stressing the loose structure of medieval industry, as well as the fiscally predatory approach that governments took to crafts and trade. But fustian seems to have required a high degree of regional and local co-ordination. The cloth could only be made if imported cotton was supplied regularly to highly skilled local specialists. Von Stromer argues that the establishment of this industry was the result of active planning by princely entrepreneurs, who worked closely with German financiers and traders to produce a cloth aimed at a mass market. This mass market was assured by establishing standard grades of fustian marked by widely known symbols of premium, good, and ordinary quality. The control of these symbols was used by municipal governments to assure consistent production. Within a generation of the Black Death, Germany had virtually driven Italian fustians off the North European market, and Italian merchants had been reduced to supplying Syrian or Egyptian cotton for German looms.

Unfortunately, there are very few written documents covering this first wave of industrial foundation. Von Stromer bases much of his argument instead on the records of the expansion of fustian production beyond its original center in Upper

Swabia. He particularly relies on King Sigismund's charter for a fustian industry at Kaschau (Kassa, today's Košice) in Hungary in 1411. Here Von Stromer argues for the coherence of Sigismund's economic policy, which was aimed at reorienting Central European trade through Hungary to the Black Sea. This might help explain Sigismund's otherwise puzzling embargo of Venice in 1412–33.

Von Stromer has constructed a central narrative that would be highly attractive to any student of the fourteenth century, but it remains a likely story incapable of direct proof. There simply are no documents to tell us how the fustian industry of Germany came into being, and all the projections backward of the established procedures of the fifteenth century will not make up for this lack. What he has established beyond a doubt is that fustian production spread rapidly and coherently in Germany in the second half of the fourteenth century, and in a way that indicates widespread entrepreneurial action. Further, he has demonstrated with documents and visual materials the cohesion of the municipal grading system. The importance of widespread standardization as a technological innovation is worth stressing, but whether this points to yet another medieval "Industrial Revolution" is another matter. His view of the vitality of late medieval German Central Europe would have been helped by the arguments of both A. R. Bridbury and B. H. Slicher van Bath on the growth of specialized industries in the period after the Black Death.

STEVEN W. ROWAN
Newberry Library

ROGER E. REYNOLDS. *The Ordinals of Christ from Their Origins to the Twelfth Century*. (Beiträge zur Geschichte und Quellenkunde des Mittelalters, number 7.) New York: Walter de Gruyter. 1978. Pp. xiv, 194. DM 88.

The term "Ordinals of Christ" is used by the author of this rather technical study to designate a list of Church offices and orders with biblical references indicating that Christ himself fulfilled each of these ministries. Thus, the office of reader or lector is accompanied, very often, by a reference to Christ serving as a reader when he read the Book of Isaiah in the synagogue (Luke 4:16–19); the order of deacon may be accompanied by a reference to Christ's diaconal role in washing the feet of the disciples at the Last Supper; and so forth. These ordinals appear in Christian literature from the fifth century on. Roger E. Reynolds gives examples in Greek, Irish, and Old English, but his study is principally about Latin versions that ap-

pear in profusion in the medieval period in theological treatises, in liturgical books, and on extra pages almost anywhere.

Different versions differed in the orders they described, in the sequence of the orders, and in the biblical sanctions they adduced. Gradually, the ranks of cantor and psalmist were dropped and that of acolyte was inserted. Whereas the earlier ordinals reflected a view of Christ as summing up, or recapitulating, in his whole life the entire life of his Church, later texts were more preoccupied with the Holy Eucharist and sought to relate the principal orders to the Last Supper. Whereas deacon, presbyter, and bishop were originally the three main orders, during the later Middle Ages the emphasis shifted to subdeacon, deacon, and priest—the latter including both the presbyterate and the episcopate.

Written primarily for the specialist, this book builds upon the work of earlier scholars and presents its information clearly. As may be surmised, the distinctive characteristics of any one source reveal influences from earlier ones and may, in turn, be reflected in later ones. Reynold's work may be helpful in establishing the provenance or background of particular texts. The comparative table at the end of this book, occupying twenty-five pages, should enable the researcher to classify any such ordinal that is encountered in a medieval text. As to the substance of these ordinals, they are not as stupid as John Calvin claimed (p. 2), neither are they likely to have any great influence on modern theological reflection on the Christian ministry. They witness very strongly, however, to the continuing Christian conviction that Jesus Christ is indeed present and working in every order of his Church, even in the lowliest and most humble.

H. BOONE PORTER
Hartland, Wisconsin

CHARLES VERLINDEN. *L'Esclavage dans l'Europe médiévale*. Volume 2, *Italie—Colonies italiennes du Levant—Levant latin—Empire byzantin*. (Rijksuniversiteit te Gent, Werken uitgegeven door de Faculteit van de Letteren en Wijsbegeerte, number 162.) Ghent: Rijksuniversiteit te Gent. 1977. Pp. 1,067.

Charles Verlinden is, beyond any doubt, the leading authority in the scholarly world today on the vast and intricate subject of medieval slavery. Twenty years ago he published the first volume of his *Slavery in Medieval Europe*, devoted mainly to Spain and France. We now have his second, large volume on the same subject, this one dealing, as the subtitle says, with Italy, the Italian colonies in the Levant, the Latin Levant, and the Byzantine

Empire. In fact, the book is far richer and more comprehensive than the subtitle implies.

It deals, first, with slavery in the barbarian kingdoms of Italy and in Italy in the period preceding the appearance of the communes. In these chapters the author inevitably has to rely rather heavily on legal texts because of the scarcity of archival materials. Beginning with chapter three, however, he moves into the later Middle Ages and there his extensive archival research and great mastery of archival documents become obvious.

After a lengthy analysis of slavery in Sicily, the Kingdom of Naples, Sardinia, Majorca, and other Aragonese possessions, Verlinden discusses slavery in central and northern Italy—in Florence, Pisa, Lucca, Sienna, Milan, and so forth. He then treats slavery, separately and in great detail, in the two most important Italian commercial cities, Genoa and Venice, and follows this with a thorough examination of the same phenomenon in the Italian colonies and zones of trade and influence: Dalmatia and Dubrovnik, Morea, Crete, Cyprus, Little Armenia, Chios, Constantinople, the Black Sea colonies, and Egypt. Finally, he devotes two short chapters to slavery in the Kingdom of Jerusalem, the Kingdom of Cyprus, Frankish Morea, the island of Rhodes, and the Byzantine Empire. In the appendixes Verlinden discusses the origins of the name "sclavus" for slaves, offers a diachronic analysis of some ethnic groups of slaves, and concludes by glancing at slavery in Italy in modern times, from the sixteenth to the nineteenth centuries.

Much of what Verlinden tells us in this book is not new, because he has published many of his results in a great number of important articles, scattered in international scholarly publications. Here we have it all assembled in one clear and excellently organized volume, easy and stimulating to use in comparing various aspects of trade, with the addition of new research, insights, and conclusions. It is, of course, an impossible task to single out even the most important achievements in such an enormous work. Still, one should point out the introduction of vast, hitherto unknown archival resources and the efficient linking of legal texts and archival documents to illustrate better the realities of life. Verlinden's mastery of the European archives is indeed impressive, even though some of his numerical results will have to be corrected here and there with the discovery of new data, just as he has corrected many earlier scholars.

Another important fact worthy of mention is that Verlinden has expanded his research far beyond the usual geographical framework of Western scholarship and has introduced new and, until now, vaguely known areas. A good example is his

detailed treatment of slavery on the eastern coast of the Adriatic Sea, based largely on the published materials from the archives of Dubrovnik. Here, again, there are some identifications to be improved, some geographical corrections to be made, and so forth, but Verlinden's great merit remains: he has broadened the horizons of scholarly research.

One might regret the absence of a general conclusion to both volumes. Such a "vue d'ensemble" from a master of Verlinden's caliber would certainly be of the greatest value and interest. Nevertheless, his monumental work will become one of the fundamental research tools for every medieval social and economic historian, as well as for many others. It should be translated into English as soon as possible.

BARIŠA KREKIĆ
*University of California,
Los Angeles*

COLIN PLATT. *Medieval England: A Social History and Archaeology from the Conquest to 1600 A.D.* New York: Charles Scribner's Sons, 1978. Pp. xix, 292. \$25.00.

Owing to our location on this side of the water, American medievalists tend to work primarily from narrative and record sources rather than to slog through muck in search of artifacts and physical remains of our medieval predecessors. Not for us the pleasant excavation of castle sites and of deserted villages, still less the exhumation of eternally grinning skeletons and of (ugh) the remains of ancestral diets as revealed in the contents of cesspits. Colin Platt demonstrates the importance of archeology to the medieval historian in a beautifully produced, well-illustrated survey; nor do I remember when I last read a book with no misprints. Platt's writing is in a few instances rather pell-mell in its fondness for detail (especially in the first two chapters), owing to the nature of his topic, but in general his style is smooth, and certainly his subject is exciting. Platt frankly acknowledges the problems and disadvantages both in his topic and in his approach (pp. xiv-xv); he stresses particularly the fact that archeological evidence accumulates so rapidly that any writer employing material remains finds his work outpaced by recent excavations before his work is set in type.

Platt's book is too rich to be summarized easily. He brilliantly weaves archeology and social history to demonstrate how people of all classes lived in medieval England; he discusses such matters as building, reclamation, diet, disease, climate, popular taste, health, religious cults, industry, population trends, productivity, public order, sources and objects of eleemosynary grants and bequests,

relations among social classes, architecture, and the distribution of wealth. His material could easily have been a mere catalogue of disconnected facts interesting only to the antiquarian; that he has presented a clearly organized, integrated discussion of a highly complex, naturally discursive range of data is a compliment to a clear and practical mind at work. Platt's treatment of the late Middle Ages and of the sixteenth century in England will comfort more the disciples of F. R. H. du Boulay (*An Age of Ambition* [1970]) than those of Johan Huizinga (*The Waning of the Middle Ages* [1949]). I found this a satisfying and fascinating presentation of the relationship between traditional history and archeology; the book is also a mine of trivia useful for enlivening lectures. I enjoyed reading this most interesting book.

JAMES W. ALEXANDER
University of Georgia

JOHN HATCHER. *Plague, Population and the English Economy, 1348-1530*. (Studies in Economic and Social History.) Cambridge: Economic History Society; distributed by Humanities Press, Atlantic Highlands, N.J. 1978. Pp. 95. \$3.25.

Among the most important, yet most difficult and perplexing, problems confronting European historians are the levels and secular trends of population in the fourteenth and fifteenth centuries and their effects on broader political, social, and economic development. Although his book is presented primarily as a summary statement of the secondary literature, John Hatcher has gone a long way toward solidifying our knowledge of the demography of this crucial, transitional period. He provides for England a brief but clear, concise, well-written, and thoughtful essay.

Hatcher begins with an analysis of the general theme of late medieval population—dwindling numbers, high mortality, and frequent incidence of infectious disease, all producing high wages, comparatively low alimentary prices, high standards of living, and far-reaching tenurial and social changes. Next, he provides a splendid historiographical survey, covering, among others, Malthusian, Marxist, and environmental perspectives (the last of which stresses the primacy of disease and other ecological factors). All are examined in admirably balanced fashion. The specifics of epidemic disease and long-term trends are covered; because of the paucity of extensive direct demographic data, Hatcher relies heavily on corroborative indirect sources, including wage and price levels and the extent of arable land.

With secular trends established—decline from 1348 through the first two-thirds of the fifteenth

century—he tries to determine why this decline was so severe and protracted. He stresses social and psychological contingencies and then compares the era of contraction with a subsequent period of renewed, albeit slight, growth from 1470 to 1525. Throughout the course of the text, he develops his major theme, the importance of exogenous epidemic disease—especially recurrent bubonic plague—in causing and perpetuating late medieval depression. In the fourteenth and fifteenth centuries, mortality was of far greater importance than fertility, marriage, or migration in affecting population levels. Yet, as is appropriate in a book devoted primarily to synthesis, Hatcher is never dogmatic. After presenting his thesis, the author includes a footnote emphasizing that his is a minority interpretation.

Although any book attempting to cover such a broad and controversial topic over close to two hundred years will have some shortcomings, they are few and far between. At some points the historiographical discussion and occasional hedging bog down the otherwise fast-moving text. The author must have been pressed for space, but more discussion of the social and economic effects of depopulation would have been helpful; and his estimates of early sixteenth century population may be a bit too low. But these and other minor problems pale beside the essay's overwhelming merits. It has a good bibliography and detailed footnotes, making it useful to both undergraduates and specialists. At one stage, Hatcher even manages to introduce into his survey some original demographic evidence taken from the Canterbury Cathedral Priory obituary list. Moreover, his discussion of wages and prices is sufficiently enlightening in its own right to make the text worthwhile. In all, this is one of the most useful books written about later medieval England in many years and should become essential reading at all levels of study.

ROBERT S. GOTTFRIED
Rutgers University, New Brunswick

Notariato medievale bolognese. Volume 2, *Atti di un convegno (febbraio 1976)*. (Studi Storici sul Notariato Italiano, number 3.) Rome: Consiglio Nazionale del Notariato. 1977. Pp. 283.

In Bologna, central to the transition from ancient to modern ways of doing business, the notaries had to be in the thick of things. They controlled the authentication of act and intention, and through their corporate body they took a leading part in politics. Besides, their work is a reflection of legal theory as well as of a significant theoretical evolution of its own. Given the complexity of the topic, it

is praiseworthy that this third volume in a series, a collection of papers on the notariate of Bologna from the tenth century to the sixteenth, can serve well as a manual.

No editor's name appears on the volume, but one may impute to Giorgio Costamagna a leading part in its formation. His contribution serves as an introduction, tracing the evolution of notarial acts from the *charta* of the tenth and eleventh centuries, whose credibility depended on extrinsic ceremony or oath, to the *instrumentum* of the twelfth century and after, which stood on the *publica fides* of the notary. He shows for the benefit of legal historians concerned with the intrinsic "content" of a document how diplomatics, addressing its extrinsic features, can raise issues not accessible to their usual procedures. The distinctions he makes continue his critique of the methodology of legal history and deserve the attention of a wide audience of historians and social scientists. Gianfranco Orlandelli's paper takes up the prospects for research on the training of notaries at Bologna, relating this topic to legal studies and to politics, among other topics. He too shows how diplomatics can contribute to legal history, demonstrating how the standard typology of documents that emerged could by itself create new legal categories. Roberto Ferrara's longer study of the examining and licensing of notaries in the thirteenth century is rich in relevant circumstantial material; Gherardo Ortalli contributes a fine review of the notaries' known and putative roles in Bolognese historiography, with many valuable insights, among them a diplomatist's view of narrative sources.

It must be said that the usefulness of this collection as a manual may be diminished somewhat by the allusive styles employed by the above authors, more congenial perhaps to the fellow diplomatist than to outsiders. No such criticism can be made of Gina Fasoli's discussion of the civic life of the notary from the twelfth to the fifteenth century, a discussion comprehensive, systematic, and clearly annotated with the interests of a wide range of scholars in mind—though the notes might have been more carefully proofread. Giorgio Tamba concludes the volume with a study of the archive of the *Società dei Notai* and of other relevant collections. This paper must be added to the short list of works that a researcher in Bologna should consult before taking up manuscript materials there. In an appendix Tamba has published in full the statute of the *Società* of 1288 with an introduction.

EVERY ANDREWS

George Washington University

GRADO G. MERLO. *Eretici e inquisitori nella società piemontese del trecento*. (Studi Storici.) Turin: Claudiana, 1977. Pp. 316. L. 7,200.

The first part of this book consists of a one hundred and fifty page essay on Piedmontese heresy of the fourteenth century; it is followed by one hundred and twenty pages of Inquisitional records in Latin. For all its brevity, and despite the geographical limitation of its subject matter, the essay ranks as a major contribution to our understanding of what medieval "heresy" really was. The traditional conception of the subject, going back to the age of confessional historiography and persisting into even the most modern works, has focused on the clarification of doctrines, the distinction among diverse doctrinal complexes, and accordingly, the identification of "heretical" groups—in short, the same questions asked by the inquisitors. Grado G. Merlo, on the contrary, picks up both the innovative approach of the late Herbert Grundmann, who suggested that we think not so much about heresy as about religious movements, and the more recent orientations of social history. Merlo shows how the heresies in question shared critical attitudes to the Church that went back to the Church's own reform movement of the eleventh century but were also developed subjectively as the self-consciousness of self-defining groups. These were not classes nor—it would seem—can they be derived from any social or political generalization. Heresy was not, as some have said, "popular religion," for orthodoxy was more "popular" still, nor can it be rigorously distinguished from orthodoxy by its ideas. Merlo seems content with the obvious: heresy was "critical religion," and it was linked with a "pre-political" religious, social, and cultural protest. His study leads to the conclusion that the protest in question was chiefly that of some of the marginal rural elements alienated from an increasingly integrated society centered in the towns. Such theses are not, however, emphasized; the book's great achievement, comparable to Emmanuel Le Roy Ladurie's *Montaillou* although on a smaller scale and with less rich sources, is to present the heretics concretely, in their social being, rather than as confessors of this or that before the Inquisitional tribunals that preserved their memory.

This is not to say that the book skimps on doctrinal definition. The Latin texts—welcome additions to our knowledge—give us all the doctrines we could want, and Merlo analyzes, compares, and classifies them. But, as he observes, the flow of ideas among the different groups, and between them and the orthodox, was constant, so that the most significant phenomenon is that of syncretism—or rather, as he notes, multiple syncretisms, with Catharan and Waldensian ideas combining and recombining, along with elements of superstition, magic, and sorcery. Catharan elements eventually lost ground, and eschatological themes

gradually became more prominent, no doubt a sign of the sectarians' objectively marginal position. The workings of the Inquisition are also studied, with due tribute to their effectiveness in suppressing some manifestations of heresy and distorting others. But the sectaries found ways to survive, absorbing new currents just as they had absorbed old ones: Hussitism and Protestantism both left their marks. We are left with the profound impression of men and women fighting to create their identities through certain kinds of religious experience; they are the given of social history, not the constructs of social theory.

HOWARD KAMINSKY
Florida International University

JOSEF FLECKENSTEIN, editor. *Herrschaft und Stand: Untersuchungen zur Sozialgeschichte im 13. Jahrhundert*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 51.) Göttingen: Vandenhoeck and Ruprecht. 1977. Pp. 328. DM 72.

This work consists of nine papers resulting from a colloquium held in September 1975 at the Max-Planck-Institut für Geschichte in Göttingen and is dedicated to Walter Schlesinger. The contributors and the titles of their papers are as follows: Josef Fleckenstein, "Die Entstehung des niederen Adels und das Rittersum"; Werner Rösener, "Ministerialität, Vasallität, und niederadelige Ritterschaft, im Herrschaftsbereich der Markgrafen von Baden vom 11. bis zum 14. Jahrhundert"; Thomas Zotz, "Bischöfliche Herrschaft, Adel, Ministerialität und Bürgertum in Stadt und Bistum Worms (11.–14. Jahrhundert)"; Gisela Meyer, "Untersuchungen zu Herrschaft und Stand in der Grafschaft Jülich im 13. Jahrhundert"; Lutz Fenske, "Ministerialität und Adel im Herrschaftsbereich der Bischöfe von Halberstadt während des 13. Jahrhunderts"; Heinz Dopsch, "Probleme ständischer Wandlung beim Adel Österreichs, der Steiermark und Salzburgs vornehmlich im 13. Jahrhundert"; Hans K. Schulze, "Territorienbildung und soziale Strukturen in der Mark Brandenburg im hohen Mittelalter"; Thomas Martin, "Die Pfalzen im dreizehnten Jahrhundert"; Sabine Krüger, "Das Rittersum in den Schriften des Konrad von Meigenberg."

The purpose of this collection is to demonstrate the reciprocal relationship between *Herrschaft* (lordship) and *Stand* (an identifiable group within the society). The chronological focal point is the thirteenth century. Heinz Dopsch, the author of the sixth paper, draws attention to the problem of terminology by beginning his study with a discussion of *Stand* (pl. *Stände*). He quotes Otto Brunner's statement that the concept of *Stand* is purely

formal: "er [Ständebegriff] empfängt seinen konkreten Sinn jeweils von der Ordnung, der er angehört" (p. 207). As Johan Huizinga observed, "The idea of an 'estate' [*Stand*] is not at all limited to that of a class; it extends to every social function, to every profession, to every group." In order to ascertain the make-up of an emerging order or estate, the authors examine the constituent elements of such terms as *nobilis*, *vasallus*, *miles*, *ministerialis* and the variety of roles that each displays in different regions. Dopsch demonstrates that in the thirteenth century (within the territories specified) one can discover a greater degree of social mobility as well as the formation of new groups (*Stände*) than is the case in the subsequent two centuries and that, furthermore, this social mobility held good for both lay and clerical orders (p. 211). Although many readers may already be familiar with the political conditions that made for such mobility, for example, the collapse of the Hohenstaufen dynasty and the consolidation of the power of the territorial princes at the time of the great Interregnum (1250–72), Dopsch's contribution is to pinpoint this phenomenon by presenting the reader with a highly informative graph that illustrates the social mobility of between thirty-five and forty families in Steiermark (p. 231).

Fleckenstein, the general editor, begins this study by noting the significant regional variations in the transformation of the *milites* and *ministeriales* into a knightly order (*Ritterschaft*). In Swabia, the *miles* moved from the position of a lesser vassal to that of a greater vassal without the *ministeriales* playing a significant role, in Thuringia the development of a knightly order was largely dependent upon the various *ministeriales*' families since as a group they were still quite numerous (pp. 36–37).

Doubtless, one of the most useful aspects of this collection is that it provides a model for those scholars who wish to integrate the precision of local and regional studies in family history, genealogy, and institutional history into the larger theme of social change. If the picture presented is still incomplete, this in no way reflects upon the present volume. Rather it supports the contention of the editor that additional work will yield a comprehensive view of the various orders and, it is to be hoped, will fully provide the first step toward a synthesis between the work being done in Germany and that which is now being carried out in France and Belgium.

BOYD H. HILL, JR.
University of Colorado,
Boulder

KARL TRÜDINGER. *Stadt und Kirche im spätmittelalterlichen Würzburg*. (Spätmittelalter und Frühe Neu-

zeit, Tübinger Beiträge zur Geschichtsforschung, number 1.) Stuttgart: Klett-Cotta. 1978. Pp. 193. DM 54.

The study of late medieval German towns has received great impetus over the last generation. The work of Bernd Moeller has been particularly stimulating in raising new questions and suggesting fresh lines for research, such as the role of the late medieval town in the coming of the Reformation. American scholars such as Miriam Chrisman and Steven Ozment have recently taken up this theme with considerable success.

Karl Trüdinger's monograph launches an ambitious series, edited by Josef Engel, that will examine the interaction of Church and state in the important communities of southern Germany from the late Middle Ages into the Reformation era. Although the imperial free cities have attracted most attention, Trüdinger's book focuses upon the exceptional case of the episcopal free city of Würzburg. For, even as the large majority of late medieval towns in Germany were throwing off the political authority of their feudal and ecclesiastical lords, Würzburg remained firmly under the control of its prince-bishop. The local citizenry saw its hopes for autonomy dashed by the bishop's decisive victory over rebel townsmen in 1400.

During the following century successive prince-bishops, ruling from the imposing Marienberg fortress above the city, secured their position further by a variety of asserted rights and customs that the town council proved generally powerless to modify. It was this strong episcopal position, coupled with the intense concentration of religious institutions in the city, that gave substance to the view that most citizens owed their livelihoods, at least indirectly, to the prince-bishop and to the Church.

It is within this political and economic framework that the town council and the citizenry at large managed gradually over the fifteenth century to whittle away some of the jurisdictional rights and privileges of the sovereign. This is the author's major theme. Drawing primarily upon local archival materials (council reports, tax records, Church documents) and, additionally, upon a few chronicle accounts, Trüdinger demonstrates how the council, along with individual townsmen, became increasingly involved in the actual running of some local Church institutions. The council, for example, came to serve as a board of trustees overseeing various charitable enterprises, exercising patronage rights on some endowed preacherhips, and even managing ecclesiastical property outright, as in a major church built with funds donated by a wealthy citizen.

But if this burgher involvement in Church affairs compensated to some extent for the political in-

significance of the town council after 1400, it reflects also the deep religiosity of the people. It was as members of a kind of "sacral community" that the Würzburg town council and the citizenry generally sought religious security. They did so through a zealous devotion to local cults and spiritual brotherhoods, as well as through elaborate processions and pilgrimages, all elements of popular piety that they shared with their clergy.

The picture that emerges from all this is one of increasing interpenetration of the lay and clerical societies of the city by the eve of the Reformation. Trüdinger contends that these mutual relationships can be detected across the spectrum of activities in late medieval Würzburg, whether economic, political, cultural, or religious.

Unfortunately, the author does not treat a basic issue raised by this discussion, namely, why Würzburg did not join the Protestant Reformation. This is particularly puzzling in that a leading purpose of the series, stated in the editor's introduction, is to provide a sense of continuity between the late medieval and the Reformation periods. Finally, although in places the work attempts little more than a sketch of important problems, it does provide useful materials and insights toward an eventual synthesis of the complex German urban scene prior to the Reformation. In this respect, the example of Würzburg reinforces the view that the development of the late medieval German community differed significantly not only from region to region but from town to town.

DONALD D. SULLIVAN
University of New Mexico

MODERN EUROPE

GUSTAVO COSTA. *Le antichità germaniche nella cultura italiana da Machiavelli a l'Uco*. (Istituto Italiano per gli Studi Filosofici. Serie Studi, number 1.) Naples: Bibliopolis. 1977. Pp. 389.

Enduring ideological conflicts, such as that between German barbarism and Roman civilization, may be carried on over many centuries and on different levels. In the case of the war between *germanesimo* and *romanità* at least three phases can be distinguished: the original military confrontations between Romans and northern invaders, modern investigation into and controversy over these rival traditions, and contemporary historiographical study of their literary repercussions. Gustavo Costa's book is involved in his fundamental cultural and racial dialectic of Western civilization at the second and last remove. His view of the problem, at least in chronological terms, is commendably broad, extending from the pioneer-

ing excavations of Flavio Biondo to the more speculative inquiries of the Enlightenment. It is also remarkably coherent, culminating in the "grand synthesis" of Vico, who indeed presides conceptually over much of this discussion. Some thirty scholars and publicists (most notably Machiavelli, Beatus Rhenanus, Sigonio, Varchi, Baronio, Campanella, Gravina, Giannone, and Muratori) are examined in terms not only of their positive appreciation of German antiquity and opening of an historical perspective (*prospettiva storicizzante*) but also of their contributions to the "new science" ("preparando il terreno alla grande sintesi vichiana").

The framework of Costa's analysis is above all established by the themes of translation, decline, and revival in the history of the Empire. The medieval idea of the *translatio imperii*, based on providential transferral of political dominance because of weakness or corruption, continued to be an issue in early modern history; and so did the *inclinatio imperii*, a formula that furnished the title of Biondo's most famous work and the central theme of Gibbon's. The main historical illustration was the sack of Rome in 410, which alternately illustrated the degeneration of the Romans and the vitality of the Goths. Even more than the Germanic hero Arminius, the Gothic king Theodoric was celebrated for his moderation and wisdom. One author at least (Ricci) pointed to the Protestant champion Gustavus Adolphus as Theodoric's modern incarnation, though another (Tesauro), caught up in anti-Machiavellian enthusiasm, identified him with immoral *ragion di stato*. Counter-Reformation (and "antibarbarist") critics like Campanella, on the other hand, deplored the licentious heritage of *goticismo* and favored instead a Romanist ideal of a modern (papalist or Spanish) *renovatio imperii*.

Most of this book is devoted to the positive side of Germanism ("l'idealizzazione del mondo tedesco"; "la superiorità dei paesi tedeschi"), especially the political virtues celebrated by Tacitus—who happens also to be one of Vico's renowned "Four Authors." Love of liberty, chastity, force, a certain kind of "feminism" (and, Costa might have added, innocence of usury)—these were the racial qualities that went into the myth of Germanism created by Tacitus and inflated by modern scholars beginning in the fifteenth century. Elements of this myth identified and argued by Costa include not only polemical constructs, beginning with *il goticismo machiavelliano*, but also more scholarly fictions, including *il goticismo giannottiano* and *il filogermanesimo muratoriano*.

The progeny of this "primitivist myth" was as mixed as its provenance. On the one hand there were delusions about the "good savage," some of

the less palatable racist inferences from the theory of climatic influence, and that nationalist fallacy (*boria delle nazioni*) criticized by Vico; on the other hand there existed the impetus to antiquarian and anthropological investigation, fruitful ideas about the emergence of the vernacular (though Costa neglects the discussion of Bruni, analyzed most recently by Hans Baron), and increasingly sophisticated notions about the process of historical change. In particular, it opened up a perspective on early ("primitive," perhaps "feudal"?) Rome and possibilities of a return of barbarism (Gravina's *barbarie d'artificio* and Vico's "second barbarism"). All of these possibilities and half-opened trails were explored by Vico in ways that were much more systematic—*storicistico-sociologica*, says Costa—if not more scientific.

All in all this is a useful and provocative book, both as a survey of an important complex of attitudes and ideas and as a contribution to the recent Vichian deluge. Some reservations may be expressed about the method, which is a running and interlarded commentary on large chunks of quotations (including untranslated Latin), about the proclivity to hypostasis (indiscriminate clustering of ideas under the shelter of some "-ism" or another), and about a certain looseness of argumentation (altogether too many—I lost count—*non per nullas*). But the discussion is knowledgeable, learned, and generally fair minded. Costa takes sides in this great ideological debate only in the sense that he values (or at least is interested in) mythology, primitivism, and cultural value judgments above historical scholarship as such. This seems in marked contrast to most studies of the classical tradition (such as Roberto Weiss's *Renaissance Discovery of Classical Antiquity*), and one is moved to ask if there is some inadvertent invidiousness in modern perspectives on the cultural legacies discussed in this book. In any case it is good to have national bias muted by the intercultural perspective chosen by Costa.

DONALD R. KELLEY
University of Rochester

JOSEF W. KONVITZ. *Cities and the Sea: Port City Planning in Early Modern Europe*. Baltimore: Johns Hopkins University Press. 1978. Pp. xv, 235. \$17.00.

Before the seventeenth century, political power in northern Europe derived from the ownership and control of land-based economic activities. As the locus of maritime trade shifted from the Mediterranean to the Atlantic, however, governments that had formerly ignored the sea began to search for ways to nurture commercial sea power for the benefit of state interests. Port city planning consti-

tuted an integral part of this program, for it not only provided for a rational arrangement of essential docks, warehouses, and transportation facilities but also made possible symbolic fusion of what *was* with what *ought to be*. Success or failure depended primarily on planners' ability to comprehend the necessity of an integrated land-sea environment for the spatial needs of commerce, housing, and industry.

This is the view presented by Josef W. Konvitz in a book conveniently divided into three chronological sections. First, he provides the sixteenth- and seventeenth-century background of two traditions in planning, the ideal and the practical. Both originated in their modern form in Italy and were given unity at the end of the sixteenth century by the Dutchman Simon Stevin. His concepts of integrating canals into the cityscape and providing for future expansion without dictating too narrowly the scope of that growth were followed most successfully in Amsterdam. But practicality in planning suffered in the hands of Swedish planners for whom shape and symmetry took precedence over fiscal and physical realities.

The heart of Konvitz's book lies in his discussion of four ports planned in the 1660s and 1670s by ministers of Louis XIV and intended to provide the basis for France's supplanting other maritime powers and assuming first place among them. Each city (Sète on the Mediterranean and Rochefort, Brest, and Lorient on the Atlantic) was assigned a different role. The planners of each had to balance and integrate the social and economic needs of the people and institutions that would inhabit the port cities with the demands of a government heretofore understanding only power and *gloire*. The interests of state and city were not necessarily contiguous, the latter serving the needs of the former. Yet all four cities survived and grew, their success assured as much by the state's knowing when not to plan as by its careful guidance.

The book's final section, a wide-ranging essay, discusses cultural changes since the seventeenth century that have taken city planning from the hands of architects and placed it in those of engineers. Konvitz issues a plea for a belated return to the concern, last fully expressed in the seventeenth century, to understand the relationship between cityscape and the sea, our earth's last frontier.

Although Konvitz has isolated the "port city," his judgments would be equally applicable to the planning of cities with other functions. His category of "port city" is itself somewhat nebulous; if we accept it as descriptive of a Sète or a Brest, is it equally applicable to a St. Petersburg or Stockholm, two cities Konvitz treats as relative failures of port city planning but which had other—and perhaps more important—functions than as ports?

Odessa would be a more suitable example of his port city. The use of the concept of "maritime culture" raises numerous questions, particularly in the book's last section. The abundantly reproduced city plans provide visual illustration for Konvitz's discussion; this is fortunate, for he tends at times to overwrite. Despite these problems, which are minor, the book makes a notable contribution to our understanding of the cultural dimensions of urban planning.

GEORGE E. MUNRO

Virginia Commonwealth University

J. V. POLIŠENSKÝ, assisted by FREDERICK SNIDER. *War and Society in Europe, 1618–1648*. New York: Cambridge University Press. 1978. Pp. 261. \$22.50.

Few if any Eastern European historians are better known and more admired in the West than Josef Polišíenský of Prague. His lectures and articles have made him familiar to most Western historians of the early modern period, and his book *The Thirty Years War* (1971) is the best treatment of the subject available in English, successfully combining a sweeping analysis of the war on an international level with an absorbing case study of the war's impact on one community in southeastern Moravia. Written from a rigorous Marxist perspective and informed by a powerful sense of Czech nationalism, the book is at the same time a work of breadth and sophistication that is likely to remain the standard treatment for many years to come.

Polišíenský has now published a second volume in English, written with the collaboration of an American historian, Frederick Snider of Ohio State University. Despite its title, this work is much narrower in scope; essentially, it is a handbook designed to describe research materials in Czechoslovakian archives and to illustrate how these resources can contribute to a fuller understanding of European history during the age of the 'Thirty Years' War.

Most of the book is organized chronologically, with chapters for each phase of the war. Each chapter begins with a brief review of the literature and a long discussion of Czech archival resources relevant to that period. The archival collections are described in remarkable detail, often down to the actual call numbers of various boxes and dossiers. Unless he or she is planning to do research in Czechoslovakia—which, of course, the author hopes to encourage—even the professional historian will find these sections of very limited interest. Browsing through them, however, one does get some insight into the complex vicissitudes of Eastern European archives, many of which passed by

inheritance from one noble family to another until finally coming under state control after 1945.

The second half of each chapter treats a specific historical problem from the perspective opened up by Czech materials. Topics range from Danish intervention in Moravia during the 1620s to relations between Bohemia and England throughout the war. But readers will not easily be convinced that the materials Polišenský has uncovered can substantially affect accepted findings. The Piccolomini papers now in Zámorsk, for example, contain some intriguing reports on English conditions in the 1640s, but they are hardly likely to lead to any revised interpretations of the English revolution. And readers of Polišenský's earlier book will already be familiar with his basic arguments—above all, that the Thirty Years' War was not a localized Central European conflict, which gradually "expanded," but rather a war that, from the very beginning, was widely and correctly perceived as a pan-European struggle against Habsburg absolutism.

The book also provides, inevitably, a review of the Wallenstein question. This section offers little that is new, except for one remarkable passage in which the author spans the centuries to link Wallenstein's enemies to Hitler's friends (p. 147): the descendants of the very noblemen who benefited from Wallenstein's fall in 1634, he argues, tended to support Czech appeasement in 1938.

For many historians, the most valuable part of this book will be the brief concluding section, which is devoted chiefly to changes in the composition of the Bohemian nobility between 1600 and 1650. This section, which draws heavily on Snider's own research, challenges the traditional view that in suppressing the Bohemian rebellion of 1618–20 the Habsburgs decimated the existing nobility and replaced it with a new "Austrian" aristocracy. According to the findings presented here, the "new" families remained in a distinct minority—although, benefiting as they did from extensive confiscations, they soon rivaled the "old" Bohemian nobility in terms of wealth and the size of estates.

This section, in fact, whets one's appetite for a much fuller treatment of the Bohemian nobility from Snider himself. But it also suggests the obvious: that Czech archives are likely to be fruitful above all for topics in Czech history. This is not to say, however, that these topics lack European significance. For that insight we are still largely indebted to the career of Josef Polišenský.

CHRISTOPHER R. FRIEDRICH
University of British Columbia

GUNTHER E. ROTHENBERG. *The Art of Warfare in the Age of Napoleon*. Bloomington: Indiana University Press. 1978. Pp. 272. \$12.50.

Historians who see the modern art of warfare as a search for more-than-Napoleonic victories with the weapons of the industrial and technological revolutions will welcome Gunther E. Rothenberg's fine survey of *The Art of Warfare in the Age of Napoleon*. It is not just another book on Napoleon and his campaigns or on Britain's war with the emperor. Nor is it confined to the efforts of Napoleon's opponents to master the new French art of war. Dealing with Europe as a whole, this work ranks with those of Peter Paret, David Chandler, and Richard Glover and is somewhat less condensed than the equally masterful survey by Michael Howard. The best comparison may be with William McElwee's equally expert *Art of War: Waterloo to Mons*.

With over three hundred thousand secondary works to choose from, this and the other general works already mentioned are based on many sources. Rothenberg has used his sources to turn out a most illuminating and readable general survey. He begins with armies and warfare in the last years of the Old Regime and summarizes the wars themselves from Valmy to Waterloo. Like Napoleon and his contemporaries, Rothenberg sees military history as a matter of armies and land operations. Blockades, embargoes, and "those far distant, storm beaten ships," which Alfred Thayer Mahan saw as standing between Napoleon "and the dominion of the world," are considered only in passing. There are several index references to foraging, armaments manufacture, and footwear, none to Nelson, sea power, navies, or the Continental System. Rothenberg might have made his view of the art of war a bit more explicit, though Mahan's claim that "the history of sea power" is still "largely a military history" is still not accepted by many maritime and military historians.

More important for the later development of the art of war, was the way those who applied the "lessons" of the Napoleonic wars saw things, and this is the way in which Rothenberg presents it. There are chapters on the soldier's trade, on France, her opponents, and on miscellaneous staff problems, fortifications, and medical services. The final chapter deals with the long-range results. About all of these Rothenberg asks the right questions. His account of the British army (pp. 173–87) begins with some general observations on the army and its place in the national life and continues with its officers, men, weapons, organization, and tactics and field supply. The account ends with its allies and auxiliaries. Not surprisingly, the major armies that were the least changed by the revolution, the Russian and British, were those whose contacts with the French had been most sporadic.

There are a few typographical errors ("Willington," p. 52); the review copy had lost its preface. Some works that I think should be in the select

bibliography were left in the footnotes. Some fine works were not mentioned in either. But any list of the three hundred best books on the Napoleonic wars is something of a parlor game. This book is well organized, well produced, and well written. It belongs among the ten most useful books on this period to the historian and, I think, to the general reader.

THEODORE ROPP
Duke University

S. V. OBOLENSKAIA. *Franco-prusskaia voina i obshchestvennoe mnenie Germanii i Rossii* [The Franco-Prussian War and Public Opinion in Germany and Russia]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 335. 2 r. 10 k.

Svetlana Valerianovna Obolenskaia has in reality presented two basic theses in her monograph on the relationship of public opinion in the German states and Russia toward the Franco-Prussian War of 1870-71. The first is the proposition that German public opinion was anything but united in relation to the war and the formation of the German Empire. The second is that especially after Sedan (but even before), Russian public opinion toward the events of the Franco-Prussian War veered sharply toward a warm sympathy with France and a corresponding hostility toward the Germans.

How convincing are Obolenskaia's theses? With respect to German public opinion, she has cited and used the major classic and most recent works of both Western and Russian scholarship, Marxist and non-Marxist. She has organized her presentation on Germany into two chapters. The first analyzes the position of what she calls the "German ruling classes" in relation to the war and concurrent events such as the Paris Commune and the International. These classes include not only Prussian Junkers, led by Bismarck and supported by the journalist M. Busch, but what she refers to as the "national-liberals" and "conservatives" in the service of Bismarck. Finally, they include the "German proletariat" supported by "German social-democracy." By citing examples of German press and other propaganda of the time (as well as other primary sources) she is quite convincing in her arguments concerning the split in German public opinion between those supporting and opposing Bismarck's policies. What she does, however, is erect a straw man by saying that "bourgeois" historians have indicated that public opinion in Germany was united in relation to the war and the formation of the German Empire. Certainly not all, or even most, Western historians would make that simplistic statement without qualifications.

Obolenskaia's second thesis concerning the public opinion of Russia toward the events of 1870-71 is supported by primary source material not only in the Russian press but from archival and manuscript collections in the Soviet Union. It is divided into three chapters: "Conservatives and the European Crisis of 1870-1871"; "Liberals on the Franco-Prussian War and the Paris Commune"; and "The Franco-Prussian War and the Paris Commune in the Estimation of Russian Democrats." She has indicated the importance of Russian conservative journalists, especially M. N. Katkov (publisher and editor of the *Moskovskiiia Vedomosti*), in the transformation of Russian public opinion from one of coolness toward France to one of warm sympathy paving the way for the Franco-Russian entente and later alliance. Nevertheless, her attack on the British specialist, W. E. Mosse, for indicating that Katkov's paper was an organ of "independent national opinion" (p. 181) is unjustified. Obolenskaia herself, as stated above, emphasized the power of Katkov's influence on governmental policy. Katkov considered himself to be the leader of state policy as indicated in his correspondence with Alexander III. The "Thunderer of Strasnyi Bul'var" never accepted a high governmental office when offered. He preferred to use journalism to influence the government through public opinion. (See Martin Katz, *Mikhail N. Katkov: A Political Biography 1818-1887*, [1966]). Obolenskaia is quite convincing in her analysis and evaluation of the attitude of Russian "Liberals" on the Franco-Prussian War and the Paris Commune, especially that of B. N. Chicherin, based on his correspondence with Stankevich. One can see the similarities and differences between the thinking of Chicherin and that earlier expressed by the father of Russian liberalism, T. N. Granovskii.

Her treatment of the Russian radical Left on the war and the Commune is equally good, if a bit emotional. It is unfortunate, however, that Obolenskaia has not used some of the major works of North American scholarship on the period, for example, those of Edward C. Thaden on Russian conservative nationalism, Effie Ambler on Suvorin, and Otto Pflanze on Bismarck.

In general, Obolenskaia's monograph, which is enhanced by a valuable bibliography and index of names, is an important contribution to the history of the Franco-Prussian War, the Paris Commune, and the impact of journalism and propaganda on European foreign policy.

MARTIN KATZ
University of Alberta

PATRICIA BRANCA. *Women in Europe Since 1750*. New York: St. Martin's Press. 1978. Pp. 233. \$17.95.
SARA DELAMONT and LORNA DUFFIN, editors. *The*

Nineteenth-Century Woman: Her Cultural and Physical World. New York: Barnes and Noble. 1978. Pp. 213. \$18.50.

Historians have viewed women's past from a number of perspectives, but perhaps the two dominant viewpoints are an analysis of women's oppression through explicit attitudinal and structural controls and a more recent focus on women's power and autonomy—as it existed outside male norms. *The Nineteenth-Century Woman* represents the first approach, while Patricia Branca's work is illustrative of the second.

Branca has contributed to the growing skepticism about an overly rosy picture of the lives of premodern women. Her work on Victorian women has also convincingly undercut the stereotype of the idle, servant-inundated, middle-class wife. When her present work builds on these strengths it is quite solid. When, however, it is written in a polemical tone that fails to acknowledge the work of other scholars, it is less acceptable. The strength of the work lies in its broad coverage and description of women's lives, although here she relies often on advice literature.

The work surveys the economic, domestic, and political developments leading to today's modern woman over the last two hundred years of European history. Branca casts her generalizations around a modernization process that dominates the book. Women, she argues, have gone through a progression from a preindustrial, traditional existence to a modern, independent, and self-interested one. They have controlled this process by adapting to any new economic structure—for instance, women chose to be servants because that occupation conveniently combined traditional settings with preparation for new industrial skills. Women, for Branca, have not been occasional workers offering themselves up to the vagaries of economic cycles. She speaks often of "feminine adaptability" to the modern work place and defines this as brief careers, nonunionization, and little job commitment. Throughout this modernization process feminists and other reformers were normally reflectors rather than instigators of change. The modern woman, she contends, supported only those feminist goals directed to her needs, and these were few.

One of the difficulties with Branca's approach arises from the basic tension between a modernization process that sweeps up all—including women—in its wake and her insistence on women's essential control over their lives. One wonders if her positioning of women in this process leaves them greater chance to buck the tide than those historians operating from a model of women's past oppression. Furthermore, there seems to be a conflict between Branca's modern

woman's self-interestedness and her family and child-centered nature. She does note that middle-class wives wanted to spend more time with their children while acquiring bottles and servants who could supplant some of their care. Certainly being a modern woman could lead in conflicting directions.

The work's major difficulty is an assumption of conscious choice where women operated with little information and fewer options. For instance, the nineteenth-century servant was less likely to choose her occupation because of its pleasing mixture of the traditional and modern than because an older relative had been a servant or because of restricted options. Even though Branca notes that servants "who could have worked closer to home in factories passed instead into the big city," there were still greater opportunities for servanthood during the 1800s.

The worth of Branca's work is more evident, however, when reading Sara Delamont and Lorna Duffin's *The Nineteenth-Century Woman*, for here is the traditional summary of the various modes of nineteenth-century misogyny. The editors focus on the educational, sociological, and medical literature of the 1800s, which defined women as physically weak and incapable of evolutionary greatness. In their preface, they turn to social anthropology for an analytical tool suited to women's past; this model, however, appears rarely in the work itself. Many of the collection's essays, especially those on medical and educational literature, are quite thorough and insightful. Yet, those knowledgeable about nineteenth-century women will be familiar with most of the ideas, if not all of the authors mentioned.

Although the editors note that anthropologists provide a subtle and sophisticated model—the existence and interplay of dominant and muted groups within one society—their recurring theme is actually the double bind of nineteenth-century women. Women were thought to be part of nature, but not of culture, and to be healthy and happy they must remain feminine; yet to be female was, by definition, to be sickly, passive, and of lower intelligence. On the other hand, masculine women, commentators warned, became infertile or mentally unstable. Anthropology, we are told, demonstrates that muted groups, to succeed, must "structure their world through the model of the dominant group." But the "Catch 22" for Victorian women meant that they could not operate successfully within or outside the dominant culture, although reformers gained greater acceptance when conforming to traditional sex roles. The muted-dominant model is not wholly applicable here, nor does it add much to the authors' textual explication and personal insights.

This collection, in discussing a number of Victo-

rian texts, often seems far from the lives of women, while Branca's work seems closer. Yet it may be that a thorough integration of the real limitations placed on women by such attitudes with a more factual account, such as Branca's, is what is truly required.

HILDA L. SMITH
University of Maryland,
College Park

BARBARA GOODWIN. *Social Science and Utopia: Nineteenth-Century Models of Social Harmony*. (Harvester Studies in Philosophy, number 3.) Atlantic Highlands, N.J.: Humanities Press. 1978. Pp. ix, 220. \$22.50.

This book is not about social science or nineteenth-century models of social harmony. Instead, Barbara Goodwin, a political theorist at Brunel University, London, concentrates on the published utopian writings of only four eccentric thinkers—William Godwin, Charles Fourier, Henri Saint-Simon, and Robert Owen—who had negligible influence as theorists of social harmony. Except for parenthetical references to the Enlightenment, the French Revolution, and industrialization, a methodological preference for "analysis" results in a treatment of the four thinkers apart from either the intellectual controversies or the complex historical events of their time. In place of systematic explanation of individual or collective thought, Goodwin substitutes an evaluative analysis of those components of utopian theories that can be tested against her ideal model of utopia. Goodwin argues that the structure of utopia must include self-sufficiency, rationality, social integration, and the satisfaction of human wants and needs. Assuming that a utopian model is a comprehensive critique of prevailing and possible social organizations, Goodwin applies her model to theories about the social uses of control, cohesion, conflict, ideology, morality, individuality, psychology, and happiness. Above all, she is interested in utopias as schemes for resolving conflict in opposition to liberal-democratic presuppositions about the value of competing interests.

Within each chapter, Goodwin addresses problems of social organization as they are discussed by contemporary social, political, and psychological theorists and by her four utopians. After an introduction that sets out the revolutionary characteristics and purposes of utopian theory, Goodwin explains the utopians' attack on the malfunctions of early capitalism as a result of their perception of incompatibilities between individuals and inhumane institutions, rather than from class conflict. Prescribing how utopians ought to think about needs, deprivations, aspirations, and ex-

cesses, Goodwin explores the utopians' reconciliation of conflict of interests through education, morality, reason, and, in Fourier's case, the liberation of the passions. In search of cohesion, the utopians rejected the organized political agencies of a legal system to follow two disparate ideological paths to similar organic ends. While Owen and Godwin began with a belief in the uniformity of human nature that concluded deductively in homogeneous, egalitarian utopias based on consensus, Fourier and Saint-Simon assumed that human diversity required stratified social structures. Through a discussion of values as social controls, Goodwin asks whether freedom is essential to happiness. Her utopians' skepticism of liberal values led them to deny that freedom involves choice and to redefine freedom as "creative activity," or social harmony somehow compatible with individual wants.

Goodwin argues that her utopians ought to be considered early social scientists, even though they fail every major test of science, because they suffered from the methodological problems endemic to the social sciences. These problems, as Goodwin sees them, result from beginning with desirable conclusions and then selecting methods and evidence to support them. But surely all varieties of social theory, not just social science, originate in visions of agreeable ends that dictate acceptable data and procedures. Goodwin concludes that her utopians were unsuccessful in persuading anyone to accept their alternatives to existing social arrangements but that they did create new Kuhnian paradigms of freedom without choice, democracy without government, and control without coercion. Although critical of the utopians' rationalist assumptions and deductive methods, Goodwin finds very attractive their organic utopias, governed by natural social harmony and reason and free from political, economic, and psychological conflicts.

REBA N. SOFFER
California State University,
Northridge

LEO A. LOUBÈRE. *The Red and the White: A History of Wine in France and Italy in the Nineteenth Century*. Drawings by MARK BLANTON and PHILIP LOUBÈRE. Albany: State University of New York Press. 1978. Pp. xx, 401. \$25.00.

For French vinegrowers the central event of the nineteenth century was the phylloxera epidemic of the 1870s and 1880s. Before this small plant louse was finally defeated, over half of the vineyards of France and nearly twelve billion francs had been lost. So dramatic was the phylloxera crisis that it

practically invites the historian to divide the modern history of the wine industry into B.P. and A.P. (before and after phylloxera). Leo A. Loubère has resisted this temptation. Without minimizing the effects of the crisis, he demonstrates that many changes preceded the phylloxera and that elements of traditionalism remained after the epidemic had passed.

The wine industry, he argues, underwent a revolution of sorts during the golden age of the 1850s and 1860s when rising demand was met by rising production. Thanks to the railroad, Midi wines could be transported cheaply to northern markets. Southern vineyards expanded accordingly both in size and in yield as quality grapes were ousted by more productive plebeian varieties. A few vinegrowers experimented with mechanization, and chemicals were used to combat the powdery mildew. Hard on the heels of this age of prosperity and innovation, however, came the depression of the phylloxera period. This crisis was a disaster for many vinegrowers and a stimulus for some change, but it did not produce a technological revolution. Although some veritable wine factories existed in the Midi, most vineyards were still too small to adopt expensive methods and equipment. Furthermore, vinegrowers had little incentive to adopt new technology aimed at increasing productivity because their problem had become one of saturated markets. More important than technological change was the precedent set during the crisis for state intervention. From that time government regulations grew by leaps and bounds while winemen for their part, suffering from surplus production, cheap Algerian imports, and frauds, began to form political pressure groups. This politicization of the industry, Loubère suggests, may well have been the most drastic change that it underwent during the nineteenth century.

The subtitle of this book is somewhat misleading. The work might be more accurately described as a history of the French wine industry with some Italian material added for comparative purposes. But, as Loubère himself explains, historians' relative neglect of the Italian wine industry accounts for this imbalance. In dealing with France he has relied heavily upon Laurent's massive work on the Côte d'Or and the intensive study of Chateau Latour edited by Charles Higounet. Occasionally, his use of the latter raises the question of how far one can generalize from a single vineyard, especially one as august as Chateau Latour. These quibbles aside, Loubère has amassed a wealth of information about every aspect of the industry including an informative chapter on the wine-making process complete with detailed line drawings. Written with wit and humor, this eminently readable synthesis should appeal to the

generalist with an interest in French wine as well as to the specialist in economic history.

PATRICIA A. LEWIS
University of Houston

BRIGITTE SCHROEDER-GUDEHUS. *Les scientifiques et la paix: La communauté scientifique internationale au cours des années 20*. Montreal: Les Presses de l'Université de Montréal. 1978. Pp. 371. \$16.75.

Brigitte Schroeder-Gudehus has written an informative and provocative book, which calls into question the traditional view that science is an international enterprise, universalist in spirit and transcultural in action. She introduces her study of the interwar international scientific community by describing and criticizing what she calls the "functionalist" vision of a future erosion of national sovereignties through the progressive integration of networks of scientific, technical, and non-political tasks. The idea that scientists are "au-dessus de la mêlée" is a cornerstone of this vision but, she argues, it is an idea little supported by scientists' behavior.

In order to make this point, Schroeder-Gudehus focuses on two interwar organizations, the Conseil International de Recherches (CIR) and the Commission Internationale de Coopération Intellectuelle (CICI). Using various archival sources, she details the history of the CIR, which was organized in 1918 by representatives of the Allies' Academies of Sciences, and of the CICI, which first met in 1922 under the auspices of the League of Nations. Both these international organizations formally or informally excluded Germans and Austrians from membership, and indeed Émile Picard at first opposed the admission to the CIR of academies from politically neutral countries on the grounds that they would lobby for the admission of the Central Powers. The special intransigence of the French and Belgian members of these two international committees is explained by national rivalries among scientists before the war, the events of the war itself, repercussions from the 1914 "Appeal to the Civilized World" signed by twenty-two German scientists, and the outcry in 1918 against the award of the Nobel Prize in Chemistry to Fritz Haber.

Where Schroeder-Gudehus's book is most original is in her description of the continued antagonism between German-speaking and French-speaking scientists at the war's end, an antagonism that led the CIR to oppose the participation of Germans in congresses, the use of the German language, and support of German publications. This boycott of the Germans was matched in turn by their "anti-boycott," in which the Verband der

Deutschen Hochschulen established rules of conduct in 1923 cautioning German intellectuals against jeopardizing the honor and dignity of the German nation by easily resuming old friendships with citizens from countries of the Entente. When approached in 1924 about German participation in the CICI, the president of the Notgemeinschaft der Deutschen Wissenschaft declined to cooperate, citing crimes committed against Germany by the terms of the Versailles Treaty.

In the end, what is perhaps most striking in this book is evidence presented that compromise and cooperation among the victors and vanquished emerged *first* in political circles and only later in the scientific and cultural groups which Schroeder-Gudehus has studied. Accords for scientific cooperation were signed between German and French ministers in 1925 long before German scientists participated in the CICI, and German scientists never cooperated with the CIR despite the insistence by the German ambassador to Paris in 1926 that this conflict in the domain of science no longer had a parallel in the political and economic sphere. Schroeder-Gudehus's occasional use of the French word *clerc* suggests that she shares Julien Benda's view of 1928 that the intellectuals, including scientific intellectuals, were betraying their transcendental and universalist calling.

As the author concedes at the outset, her study suffers in thoroughness and conviction by its emphasis on a few organizations to the exclusion of scientists' individual interactions. Too, the attitudes of a number of distinguished French and German scientists are not discussed at all, among them Perrin, Langevin, Cotton, De Broglie, Schrödinger, Heisenberg, Pauli, and Born. The book includes a useful bibliography, but its index includes only persons' names; this is especially unhelpful in a book devoted primarily to institutions and organizations.

MARY JO NYE
University of Oklahoma

ADRIAN PRESTON, editor, *General Staffs and Diplomacy before the Second World War*. Totowa, N.J.: Rowman and Littlefield, 1978. Pp. 138. \$13.50.

The six stimulating essays that comprise this little volume, originally prepared for the annual Military History Symposium at the Royal Military College of Canada (spring 1977) by historians expert primarily in diplomacy and international politics, examine the great questions of European security and order, both national and collective, through the eyes of cabinets and foreign offices as well as general staffs. D. C. Watt sets the tone by asserting that historians writing on the origins of

the Second World War have concentrated too exclusively on depicting it as yet another struggle among independent nation-states for mastery and domination, whereas many observers and participants saw the events of 1931-41, if not the entire interwar period, as a civil war covering most of Europe. In these events, which marked the second breakdown in this century of Europe as a states system and as a transnational society, military leaders played four major roles, all essentially negative: they failed to underwrite the political functioning of the League of Nations; they failed to uphold the domestic systems of their countries; they failed to keep abreast of changes in the technology of war; and they failed as Cassandras to prevent their political overlords from a new recourse to force. To be sure, political attitudes played such a role that responsibility for these failures cannot be ascribed to professional military advisers alone. But their role was nonetheless negative rather than positive; it was a failure to restrain rather than an urge toward action—in sharp contrast to their role in the events leading to 1914.

The failure of the military in Germany to serve as a brake on Hitler is detailed by Gerhard Weinberg. German military leaders miscalculated both the location of living space to be conquered and Hitler's desire for a politicized army. Cautious professionalism and determined blindness reigned supreme. A few dissenters aside, the experience of Munich increased their resolve not to expose themselves again to charges of cowardice; and in 1939 the prospect of taking Poland and coming to an agreement with the Soviet Union looked "positively dazzling." The situation in Russia, described by Albert Seaton, was much the same. The general staff abdicated its proper role to Stalin; military leaders were pliant puppets, mere cogs in the state machine who deliberately closed their eyes to the significance of the events of the time. Nor were things in Italy, according to John Whitlam, much better. Under Mussolini, bluff and propaganda replaced realistic planning, bombastic speeches and slogans the careful evaluation of strategies; and the complicity of the entire military hierarchy, content with its privileged place in society, was crucial in lending credibility to it all. So Italy remained unprepared for a serious war.

The thesis of Robert Young's essay on French strategy and diplomacy in the 1930s is that the idea of a *long* war, with vital allied (especially British) support, was basic to all French military planning. There was broad consensus between civilian and military authorities at the highest level of strategic planning; and, although the role of the general staff in specific crisis situations was "unspectacular," it did advise the government more often than the chief of staff cared to admit. Thus, French

strategy was far more coherent, more reasoned, than is often contended. Unfortunately, theory came to clash rudely with circumstance. Across the Channel in Britain, David Dilks declares, it was the attempt to meet threats in widely separated theaters that imposed acute strains, and often near paralysis, on British foreign policy. Service ministers did not agree upon strategic doctrine, and the chiefs of staff tended to submit aggregate rather than joint plans, thus leaving the policy makers uncertain. The need to "buy off" at least one potential enemy (Italy) was wholly supported by the military planners. Indeed, appeasement in 1937-39 was a policy heartily approved and frequently recommended by the service departments. British confidence improved in 1939, but like French theory, it was rudely shocked by German power and efficiency.

In all of these essays, specialists can find interpretive slants with which to take issue: for example, Seaton's contention that Russian policy in 1939 aimed only at the collapse of international order, or Dilks's assertion that the limits set by the Treasury upon British rearmament were reasonable and unavoidable. But the major theme of the collection, that European military leadership failed to play a constructive role in the course of events leading to the Second World War, is incontrovertible; and all of the major nations of Europe must stand indicted in some measure.

WILLIAM R. ROCK
Bowling Green State University

GEORGE L. MOSSE. *Toward the Final Solution: A History of European Racism*. New York: Howard Fertig, 1978. Pp. xvi, 277. \$17.50.

Awareness of race was born in the eighteenth century, out of the Enlightenment by national conceptions. On one hand the desire to categorize and rationalize and on the other the aspiration to unity within a common fatherland sparked interest in origins. The organic nation sought out its roots in race, defined by linguistic, anthropological, esthetic, and historical criteria and by a host of more or less spurious sciences, including physiognomy and phrenology.

George L. Mosse connects this with the pietistic revival of the Christian cults, which stressed a community of like-minded people joined by common symbols, rituals, and emotions in an inner fatherland of Christ, easily confused with the national community. He notes French priority and longstanding supremacy in racist thought. But he stresses above all the interpretations suggested by would-be scientific studies supposed to further illuminate historical destinies already revealed by linguistics and history and leading to the develop-

ment of eugenic doctrines. For serious and humane physicians like Cesare Lombroso this could suggest that some men (for example, habitual criminals) were unredeemable. For a biologist like Ernst Haeckel, the logical conclusion was the elimination of degenerate and unproductive social elements. It would be left to German National Socialists to put such conclusions into practice, first in the 1933 law for the prevention of hereditary sickness and in the euthanasia program of 1939-41 that killed some seventy thousand incurably sick, then in the massacre of "degenerate" and "antisocial" elements like the gypsies and, above all, the Jews.

Racist literature had not consistently attacked the Jews. For the like of Gobineau and Renan, it was Orientals and blacks who represented the inferior, and potentially dangerous, races. Many writers classed Jews with the superior Aryans or Caucasians. But the anti-Jewish current was too strong, the Jew too near and too *available*, the anti-Semitic tradition too well established (religious tradition reinforced by economic and social resentments), and the anti-Semitic obsessions of a few too virulent. Mosse plausibly attributes the success of the "scavenging ideology" he studies to its banality and eclecticism, confirming argument by visual references, taking the commonplaces of virtuous life and enlarging them with racist arguments to "achieve new and horrifying dimensions." It is worth remembering, though the book does not dwell on this, that such horrors were perpetrated or, at least, first conceived in what "the true believers" deemed to be a good cause.

Theory, myth, and mystery met in a potent mix: the theories providing convenient rationalizations for prejudice; the myths operating as self-realizing descriptions of realities they helped create. Here, too, Mosse is convincing: "Racist myths not only explained the past and brought hope for the future, but through their emphasis on stereotypes rendered the abstract concrete." Stereotypes (or caricatures) brought theory to life and, sometimes, turned arguments into self-fulfilling prophecies.

Mosse, of course, knows all that there is to know about *völkisch* and anti-Semitic ideologies. He has given us a brief, convenient, encapsulated history of the racial idea, of its ideological cousinage, of its relations with rival or related movements and ideas. He touches on racist views as they applied to other races, but only glancingly. European racism, in his book, shakes down into anti-Semitism and culminates in the Holocaust. But, while anti-Jewish racism rages on, this is only one branch of European racism that, in turn, has spawned racist ideologies among other races no less potentially destructive than the original version.

EUGEN WEBER
University of California,
Los Angeles

JOHN PATTEN. *English Towns, 1500-1700*. (Studies in Historical Geography.) Hamden, Conn.: Archon Books. 1978. Pp. 348. \$25.00.

John Patten's *English Towns, 1500-1700* promises to communicate ideas about preindustrial towns from a practitioner of one discipline to those of another. Patten is an urban geographer, and this work wears the silks of Dawson's "Studies in Historical Geography" series. Like Peter Clark and Paul Slack, whose *Crisis and Order in English Towns* (1972) has done so much to stimulate recent inquiry, Patten courageously adopts a comparative approach to the topic. In five lengthy chapters he emphasizes the processes of "change" as opposed to growth *per se*, the basic economic and demographic fragility of the town, and its integral relationship—save for London—with surrounding countryside. In a final chapter Patten applies his themes to an analysis of urban structure in East Anglia, a region that he ungenerously, but wisely, limits to Norfolk and Suffolk.

Patten writes with deliberate caution. His population estimates for London and England linger at the very lowest edge of accepted consensus. Examples of towns that doubled in size are quickly countered with examples of others that declined. The very definition of a town itself that Clark and Slack employed to the apparent satisfaction of most historians is rejected as too precipitous.

Though founded on an obviously wide reading of sources, some of Patten's conclusions are open to alternative interpretation. Patten advises the reader, for example, against the view of rapid growth in urban population and the assumption of extensive economic specialization as significant themes of urban change in this period, preferring to place "real" advances of this type beyond 1700. But what may not seem real to Patten might have struck contemporaries as very real indeed, especially against the familiar experience of demographic and economic stagnation that seems to have occurred through the later Middle Ages and on into the early sixteenth century. This relativity of perception raises the question of how far one may usefully generalize about such a complex issue. Does it facilitate our understanding of severe population pressure in a number of Tudor and Stuart towns, or the important steps to remedy that problem, to have emphasized that the national rate of urban growth remained moderate? I think not, but if we accept the general terms that Patten proposes, we may easily overlook that such problems existed and that crucial developments in the formation of municipal institutions arose toward their solution.

Finally, the historian may be disappointed in seeking insight from an ancillary discipline. Though Patten's interpretations are often new and

always worthy of serious consideration, much of the ground he covers is familiar, and the light he might have shed on such less familiar themes as the spatial organization of towns, or the locational preferences of their inhabitants, is left for future work. What remains is scholarly and provocative but largely within the bounds of what historians do themselves.

ROBERT TITTLER
Concordia University

PETER SUTCLIFFE. *The Oxford University Press: An Informal History*. New York: Oxford University Press. 1978. Pp. xxviii, 303. \$15.00.

Publication in 1978 of two works of extraordinary quality appropriately marked the five hundredth anniversary of the Oxford University Press. Nicholas Barker provided an illustrated history, in which both commentary and plates absorb one's attention, in his *The Oxford University Press and the Spread of Learning 1478-1978*. And in the work under review Peter Sutcliffe has given us "an informal history," largely devoted to developments since 1860. To a considerable degree both were inspired by volume one (to 1780) of Harry Carter's monumental *A History of the Oxford Press* (1975).

In each of these works "history returns a long and rambling answer," in Carter's words, to the question, "What of the Oxford Press?" The result in the Sutcliffe volume is an episodic account largely devoted to the impact "of a few towering personalities," with the course of events determined as much by circumstance as by design. Searching for continuity, we find it, in a sense, in the long history since 1633 of the committee known as "the Delegacy" but more significantly in the efforts of the press over the centuries to accommodate to change in the social and intellectual world. Sutcliffe's achievement is in recreating this atmosphere, which defies abridgement or generalization.

The story begins in 1478 with Theodoric Rood, an itinerant printer from Cologne, from whose press in Oxford seventeen books survive. There is little continuous history until the seventeenth century when William Laud, chancellor of Oxford as well as Archbishop of Canterbury, with his adherence to tradition and his interests in texts, religious and classical, set the tone of the press for the two following centuries. Briefly, during the Restoration, John Fell was able to associate the press with the contemporary world of learning, and early in the eighteenth century "came the first big success" in publication of Clarendon's *History of the Great Rebellion*. But for the next one hundred and fifty years the learned press was dominated by the Bible press.

Sutcliffe's account begins in detail in 1860 with the career of Bartholomew Price, associated with the Delegates for nearly forty years; more than any other one individual he is responsible for the press as it is known today. Of course the Bible and the Prayer Book remained the best known of Oxford books; a million copies of the *Revised Version of the New Testament* were ordered in advance of publication (1881), but in the end it was grossly overprinted. Books were now published as well as printed, and learning and education were happily united. After long planning and a considerable degree of patience, the famous series took shape. These included the *Oxford Classical Texts*, the *English Texts*, and the English, German, and Irish *Verse*; histories (Stubbs, E. A. Freeman, Hodgkin and eventually the *Oxford History of England*); dictionaries, especially the *Oxford English Dictionary*, a task of some seventy years, with the twelfth and final volume appearing in 1928 and the set defining more than 414,000 words; the Fowlers' *King's English and Modern English Usage*; and the *Dictionary of National Biography*, acquired in 1917. At the same time the press expanded its market, first to the United States and then to Canada, Australia, India, and South Africa.

Since the theme is constantly changing, readers, at first, might expect their attention to flag. Quite to the contrary. The detail commands attention and provides a very human story told with a candor and humor that is refreshing, at times startling. The result is a work that few students of British history, and indeed few readers with taste, will want to miss.

ALFRED F. HAVINGHURST
Amherst College

G. D. SQUIBB. *Doctors' Commons: A History of the College of Advocates and Doctors of Law*. New York: Oxford University Press. 1977. Pp. xiv, 244. \$23.00.

In recent years there has been a growing awareness of the role played by civil lawyers in English history, especially in the Tudor and early Stuart periods when the civil-law courts seemed for a while to present a very real threat to the continuing effectiveness of their older, common-law rivals. Anyone at all interested in the subject has known that Doctors' Commons was for the civilians pretty much what the Inns of Court were for the common lawyers, but it has not been easy to find out much more than that about the former institution. The publication of this book by G. D. Squibb now makes readily available to historians all that they might conceivably need to know about the topic.

In his opening chapter on "The Early Years"

the author demonstrates conclusively that the date ordinarily given for the founding of Doctors' Commons, 1511, is too late; it certainly existed in the mid-1490s, and it was probably informally organized just a few years before that. He explains how some major misunderstandings arose concerning the early history of the institution because of a mistaken use of the major documentary source available, the Subscription Book, which was "not a register, nor . . . an account of money received, but a collection of legally enforceable obligations" (p. 11).

Subsequent chapters discuss the society's membership, constitution, dining hall, church, and other buildings during the nearly four centuries of its corporate existence. The 109 pages of text are supplemented by a further 103 pages containing five appendixes: the first lists and discusses the society's surviving records, most of which are to be found in the Lambeth Palace Library; the second gives a list of the presidents, which is fairly complete from 1560 onward; the third provides a list of all the known members, with a brief biographical note on each of them; the fourth is a similar list of all civilian judges and advocates in the sixteenth and seventeenth centuries who are not mentioned as members in the surviving records; appendix five gives the full text of the first and only charter of incorporation ever granted to Doctors' Commons, by George III, in 1768.

Just ninety-seven years later, after common lawyers were admitted by statute into all of the jurisdictional preserves formerly reserved to civilians, the twenty-six remaining members of Doctors' Commons voted to sell off their corporate property and to share equally the £79,000 that they received. The society remained alive on paper down to 1912 when the last surviving member died. Now Doctors' Commons lives again in the pages of Squibb's book. All students of English legal history will want to read it, and all serious students of the Tudor and Stuart periods should. So should anyone interested in the history of the City of London.

MICHAEL DE L. LANDON
University of Mississippi

ROBERT S. GOTTFRIED. *Epidemic Disease in Fifteenth Century England: The Medical Response and the Demographic Consequences*. New Brunswick, N.J.: Rutgers University Press. 1978. Pp. xiii, 262. \$17.00.

This study employs about twenty thousand probated, registered wills and other similar evidence from the Bishopric of Norwich, in an attempt to analyze the effect of epidemic disease, especially plague, on the people of East Anglia between 1430 and 1480.

With such an enormous amount of material, the author turned to the computer for assistance in organizing the data and in working out a quantitative analysis based on that material. Essentially, Robert S. Gottfried has spent a great deal of time, and 245 pages, to illustrate that the demographic patterns in East Anglia were just about the same as described in contemporary literary sources. Of course, in doing so he has made one contribution. He has shown that J. F. D. Shrewsbury's *A History of Bubonic Plague in the British Isles* contains some questionable viewpoints on the effects of the plague between 1430 and 1480.

Shrewsbury felt the plague was not all that destructive in the fifteenth century, but Gottfried's analysis places Shrewsbury's assessment, as well as that of J. M. W. Bean, in some doubt. The evidence cited by Gottfried indicates that, between 1430 and 1480, there was a serious demographic problem in East Anglia, as population seems to have declined until about 1470. That the actual cause of the decline was plague seems reasonable, especially since the critical periods of mortality coincide with contemporary reports of epidemics. Nevertheless, the wills do not actually state the cause of death (p. 118) and this inevitably will allow those who wish to dismiss Gottfried's thesis to do so with at least some justification. Gottfried has a very good circumstantial case, but he has been unable to prove his point conclusively, simply because of the nature of his evidence.

Gottfried's analysis of the relationship between plague and social structure is very weak. He has been unable to carry out any meaningful analysis of how epidemic disease affected the various segments of society, especially the rural population. His study offers virtually no insights into how the Black Death affected the social organizations and patterns of life in villages and towns. Indeed, the author seems to be unaware that the vast majority of the population was comprised of "peasants" (p. 29) of widely varying economic circumstances. He thus never asked himself how the social structure of villages might have changed during the period. To be sure, Gottfried's sources do not tell him much about social status, but it is also a topic that he seems reluctant to explore, even in a general way.

In the final analysis, a study such as this, which can offer but a superficial and, to a certain extent, debatable estimate of population trends, is of limited value as a work of history. It may be of help in a future synthesis, but on its own it is merely a rather unimaginative academic exercise. One may be critical of the likes of M. M. Postan (p. 225), but Postan has attempted to establish the social and economic relevance of his findings. Even Shrewsbury, whose scholarship Gottfried finds so

wanting, tried to relate his study to fifteenth-century life. Demography is essentially a numbers game. It can only become a useful branch of history when the actual social and economic ramifications of population trends are examined. Perhaps the author will do this in a subsequent study. Indeed, he may even discover that Shrewsbury's positive assessment of the social and economic life of fifteenth-century England is correct, but for the wrong reasons. For all Gottfried knows, the declining population in East Anglia may have had dramatic positive effects on the social and economic structure of rural society.

EDWARD BRITTON
Bracebridge, Ontario

VICTOR SKIPP. *Crisis and Development: An Ecological Case Study of the Forest of Arden, 1570-1674*. New York: Cambridge University Press. 1978. Pp. xii, 132. \$14.95.

For years English historians have been aware of a major demographic expansion that took place between 1500 and 1700, but few have ventured to present detailed studies to help us understand how local preindustrial societies were able to support their growing numbers. Victor Skipp's slim volume shows how five parishes of northern Arden in the eastern Midlands adapted to this change.

Mid-Tudor Arden was characterized by a small population living in ecological equilibrium with its predominantly pastoral economy. Concomitant with the rise in local population after 1570 there was a shift in primary and secondary land use in these Worcester parishes. Skipp shows from an analysis of extant inventories that lands were reconverted to more labor-intensive uses—more arable cultivation and emphasis on dairying and cheesemaking rather than beef production—and that there emerged a growing body of full-time landless craftsmen. New agricultural techniques, peaceful enclosure, convertible husbandry, and the growing of more and cheaper grains as well as the development of small craft industries all helped sustain the rising population. By the mid-seventeenth century under this "new ecological regime" the population threshold for Arden was 50 percent higher than it had been eighty years before. Skipp's overall historical analysis of these decades is summarized in a schematic model of the demographic, economic, and social developments in Arden (pp. 66-67).

As the title suggests, the development of economic diversity was not paralleled by a steady, progressive demographic transition. Both the 1590s and 1610s witnessed food shortages that resulted in death from famine, a temporary decline in fertility,

and postponed marriages. I am not completely satisfied that Skipp presents a wholly convincing case when he argues that the demographic character of the crisis of 1613–19 was worse than that of 1594–98. Nevertheless, these crises do show that, despite economic changes going on since the 1570s, Arden was not yet able to support a growing population when harvest complications arose. By contrast, the impact of later crises was comparatively milder.

In spite of the growing body of work on English regional economies, scant attention is given here to outside forces that helped shape the agricultural changes in Arden. On the other hand, this study points out that the timing of demographic crises may vary from locality to locality and that it was sometimes associated with a chain of events originating in distant communities. Skipp also puts into better perspective the factors that helped determine a particular community's reaction to ecological difficulties. Each varied in the availability of waste land for settlement, the land policies under the local seigniorial and tenorial system, the ability to produce more cheap food, and the presence or proximity of resources for the development of industrial employment. Although Arden was able to adjust with a minimum of strain, the transition was longer and more difficult for many other local societies.

DAVID GRAYSON ALLEN
Massachusetts Historical Society

ANDREW B. APPLEBY. *Famine in Tudor and Stuart England*. Stanford: Stanford University Press. 1978. Pp. 250. \$14.50.

Since Goubert's classic work on the Beauvaisis, French historians have given much prominence to *crises de subsistance* in the early modern period. Most English historians, however, faced with the existence of demographic crises, have preferred disease to famine as an explanation, and in 1965 Peter Laslett could entitle a chapter of *The World We Have Lost* "Did the peasants really starve?" It is thanks to Andrew B. Appleby more than to any other scholar that the question can now be answered in the affirmative, and his book represents a major addition to our knowledge of early modern England.

The core of the work is a study of three subsistence crises in Cumberland and Westmorland (roughly the new county of Cumbria), which he published in preliminary form in 1973. "In the short period from 1587 to 1623," he argues, "there were two years of famine, 1597 and 1623; one year of plague, 1598; and one year of typhus probably aggravated by famine, 1587." The analysis of the 1587 crisis is perhaps a little tentative, but those of

1597–98 and 1623 are abundantly justified. The argument is thorough and persuasive and buttressed by ample documentation from parish registers and population counts. The analysis is preceded by a lengthy regional study of Cumbria setting the crises in their immediate social and economic context and followed by chapters setting them in a wider English and European framework. He adduces changes in the Cumbrian economy that made it less vulnerable to famine after 1623, including a considerable population decline (a startling 37 percent if the calculations are correct) between 1603 and 1670–88.

Cumbria was an upland region with much infertile soil, which Appleby is the first to admit was very different from much of southern England. Nevertheless, there is enough in the way of national context and conclusions to justify "England" in the title of what is much more than a regional survey. He suggests that 1597 was the last nationwide famine (although he later adds the fascinating suggestion of a completely neglected nationwide famine in 1638–39) and that the crisis of 1623 "was localized in the North." This may well be true as a broad generalization, although there is explicit record of starvation in Lincolnshire in 1623, and another recent study argues for a mild crisis of subsistence in north Warwickshire in 1613–19 (V. Skipp, *Crisis and Development* [1978]). The chronological pattern of national and regional crises clearly raises many more questions; but that is the merit of any pioneering study, and Appleby has certainly provided an indispensable guide to an important and neglected subject.

D. M. PALLISER
University of Birmingham

R. BUICK KNOX, editor. *Reformation Conformity and Dissent: Essays in Honour of Geoffrey Nuttall*. London: Epworth Press. 1977. Pp. 302. £8.50.

This was in conception a thoroughly appropriate *Festschrift* effort to honor the scholarship of Geoffrey Nuttall, the focus of which has been resolutely theological, devotional, and clerical. A number of distinguished historians contributed essays of somewhat broader historical gauge, but the overall emphasis is faithful to Nuttall's ministerial pre-occupations.

In addition to a journeyman appreciation and bibliography of Nuttall's life and publications, there are twelve more substantial essays, three of which have something significant to contribute to the history of religion in early modern France. The biggest disappointment (for all but seminarians) among the undistinguished pieces by distinguished scholars is that by Owen Chadwick, Regius Professor of Modern History at the Univer-

sity of Cambridge. His effort to explain how it came to pass that the terribly uninteresting Frederick III, Elector Palatine, became a Reforming prince resolves itself into the unanticipated, indeed accidental, circumstance of his marriage to a Protestant woman. Chadwick concludes that none of the incredibly complex events within the Holy Roman Empire determined the religious fate of Heidelberg, which was sealed rather by the dull-witted evangelical faith of Frederick's marital inheritance "intent on truth."

Leading the nonseminarian contributions to this collection is an essay by A. G. Dickens on Johannes Sleidan, who was the author of not only the first history of the German Reformation but also much the best to be published before that of William Robertson appeared over two centuries later. There are all sorts of interesting angles from which to examine Sleidan's thousand-page folio version of the progress of heresy from Luther to the Schmalkaldic League, and Dickens (briefly at least) explores most of them. He is understandably impressed by the fact that the first history of Reformed Christianity was the work of a pagan. Dickens uncovers the complex humanist sources of Sleidan's conceptual scheme that may have been derived from Polybius, certainly was influenced by his stylistic admiration for Caesar, and unfortunately owed much to the Book of Daniel by way of Melancthon in seeing modern history as an appendage of Roman history.

Sleidan's *Commentaries* was at once philosophically radical and astonishingly popular—or at least a publishing success: before 1560 there were two Latin editions, two German editions, a French, Italian, and very successful (three issues) English edition. Yet this was the historian who, in the "Apologie" of John Daus's translation, set out as his interpretive line that "in describing matters of Religion, I might not omitte polytique causes, for . . . they came in maner alwayes together, and especially in our tyme they could not be separated." He was also one of the first to write a kind of "cultural history," while his "scissors-and-paste rendering of pamphlets and official documents" he professed to be governed by a prescript in Latin that precisely anticipated Ranke's famous *wie es eigentlich gewesen*.

Patrick Collinson's contribution is well worth reading but is difficult to review. It is a re-creation of a Puritan idyll set in beautiful Elizabethan Suffolk, where he finds "there may have been a closer approximation to the type of a godly commonwealth than in any part of England at any time: closer than anything achieved in the years of ostensible puritan triumph, which was also a time of confusion and division."

The third essay of real value is that by Christopher Hill, in which he looks for the historical roots

of that "occasional conformity" that was so major a political, ecclesiastical, and social issue in the late seventeenth and early eighteenth centuries. I was delighted to discover that Hill's analysis of the issue leads him to a reconstruction of the history of Puritanism that is closer to my own view than anything he has previously published. Beginning with "Calvin's object . . . to show that true religion is completely consistent with the maintenance of private property," Hill argues that "when the bishops' campaign of the fifteen-eighties and nineties forced Puritans to choose between submission and separation, most educated, middle-class responsible Puritan laymen chose the former and so avoided being forced into the company of seditious sectaries."

This choice was repeated in 1662 as a "reunion of the natural rulers against the radical lower orders," when the lay descendants of the old conforming Puritans turned to the policy of purging the radical (and socially, always the poorer) classes from the state bureaucracy and the parliamentary national church. The "balanced constitution" after 1660 required an unreformed state church that lost (forever as it turned out) the Elizabethan ambition of national comprehension and soured Nonconformity into the provincialism of the eighteenth and nineteenth centuries. Thus the "church and king" mobs that bawled for Sacheverell in 1709 came from those despised precincts deserted by the Church of England that had "survived primarily because of its deep roots among the landed ruling class . . . but also because 'the rabble' preferred its conservative laxity to an enforced Presbyterian discipline, or the voluntary self-discipline of the sects."

Hill apologizes at one point in this lecture (which he delivered originally to the United Reformed Church History Society, of which Nuttall was first president) for "what some may consider excessively sociological terms" in his analysis; I think most readers will approve both the terms and the skill with which the master historian employs them to make sense of a very puzzling story.

C. H. GEORGE

Northern Illinois University

ALICE G. VINES. *Neither Fire nor Steel: Sir Christopher Hatton*. Chicago: Nelson-Hall. 1978. Pp. xvii, 248. \$13.95.

In thirteen chapters, some topical, some chronological, Alice G. Vines has attempted a new biographical study of Sir Christopher Hatton, Elizabethan courtier and favorite. Hatton became lord chancellor of England in the course of a public career that began, in a minor way, perhaps as early as 1561, assumed more significant propor-

tions in 1577, and continued until his death in 1591. Several early chapters, which deal more with public affairs than with Hatton, add little to our general understanding of Hatton's role in government. The effort falls regrettably short of success.

Although it is undoubtedly true that Hatton has not enjoyed the attention given by serious historians to Burghley, Walsingham, Leicester, and Essex, we are not told why Vines has decided to redress the balance. One reason for Hatton's neglect may be the scarcity of evidence. Vines covers his family background, early years, and education in no more than eighteen lines; and she is forced, thereafter, to rely heavily upon two previous studies—the earlier, a letter book and an important source. Eric St. John Brooks made a valiant effort to do Hatton justice in a 1946 biography, which Conyers Read, in his *Bibliography of British History: Tudor Period* (1959), termed inadequate. It is difficult, however, to discover significant improvement in the present study. Indeed, there are many ways in which Brooks's Hatton has the better of it. Brooks, in much more graceful prose, provides valuable appendixes, more information about Hatton, and information about the sources—all of this alongside a more balanced view of him. Surely it cannot be agreed, as Vines suggests, that Hatton was “the Queen's right-hand man . . . in politics and religious policy . . .” (p. 30). This might be said of Burghley or Walsingham, but certainly not of Hatton. In her account of Hatton's income Vines overlooks the significant role of gratuities, a much discussed Elizabethan phenomenon. She also gives Hatton's great manor house at Holdenby too little attention. There is, in short, little to commend the book, and there are no major interpretations at variance with Brooks. Most discouraging to the reader, however, is the writing, abounding as it does in eccentricities and infelicities. Inadequate though it may be, anyone interested in Hatton will be better advised to consult Brooks's study, which remains the best we have.

RICHARD C. BARNETT
Wake Forest University

WILLIAM INGRAM. *A London Life in the Brazen Age: Francis Langley, 1548-1602*. Cambridge: Harvard University Press. 1978. Pp. ix, 335. \$17.50.

Some men are born to trouble others. Such a man was Francis Langley. As another younger son of an already large rural family, Francis troubled his parents. He was lodged in London from the age of eight with an ambitious uncle, whom he embarrassed by misbehaving in the Company of Drapers. Through schemes that, if not scandalous, were

at least dishonest, he inflicted loss on those to whom he loaned money. He established himself as a landowner by taking advantage of others' indebtedness. As a City officer he used his authority to inspect cloth in order to practice extortion. He quarreled with his parish over payment of the tithe. He was charged more than once with assault. He took advantage of family and civic connections when it suited him, but he never recognized the reciprocal claims of these ties. But the wheel of fortune turned both ways, and Langley took as much pain as he gave. Incidentally, he built the Swan playhouse on his Southwark manor called Paris Garden.

On the strength of his acquaintanceship with Langley, William Ingram indulges himself in the convention of age integration by deciding that Langley was “in many ways the embodiment of his times” (p. 7). Fortunately, his nod to this convention is just that, for Ingram has produced a thoughtful and satisfying biography of a unique and outrageous Elizabethan, which has the added benefit of demonstrating how biographies can be done against the backdrop of imperfect documentation, a typical problem for the early modernist.

The author's method is to squeeze as much juice from each aspect of Langley's life and career as the scanty documents permit and then to elucidate each by placing it in the larger context. Naturally, this method creates a tension between a sharply focused subject and his necessarily more blurred background; the balance is not always successful. The Cadiz expedition and the affair of the diamond (stolen from a Portuguese carrack) receive far more attention than Langley's slight participation deserves. The production of “The Isle of Dogs” may or may not have been connected with the demise of the Swan. Even after a lengthy recital of the evidence we are uncertain, and subplots on censorship and the players themselves never develop.

On the other hand, the author is mostly in tight control of his material, and his discussion of financial practices in the City, of manorial landholding in a suburban setting, and of the origin and early years of the Swan are richly rewarding. Best of all, we have a vivid and vital portrait of a human life, which asserts itself with sufficient complexity to defy categorization and detail enough to command credibility.

Implicit in the book are some refreshing attacks on the heroic clichés surrounding the Elizabethan age. What comes through is petty greed and contentiousness, the appalling delay in transacting any business, the frustrations and indignities of daily life, the constant threat of personal ruin, the inefficiency of local government, and the remorseless “big brotherism” of the Privy Council, led by

that ubiquitous and hideous octopus, Sir Robert Cecil.

FRANK F. FOSTER
Denver, Colorado

GEORGE H. WILLIAMS *et al.*, editors. *Thomas Hooker: Writings in England and Holland, 1626-1633*. Cambridge: Harvard University Press. 1975. Pp. viii, 435.

This edition of Thomas Hooker's early writings is not complete, omitting as it does a major work, *The Souls Preparation for Christ* (1632). But it includes a representative collection of sermons and other occasional pieces that survived more or less fortuitously from Hooker's ministry in England and from Amsterdam. They contain the earliest extant formulations of his thought in two areas of significance that he subsequently described more definitively and voluminously in America: the process of regeneration, or *ordo salutatis*, and church polity, later expounded as a version of the "New England Way" in the posthumously published *A Survey of the Summe of Church Discipline*. Two of these early works are also of great interest and appeal as a reflection, from the lips of this most affecting of Puritan preachers, of that premigration mentality that confronted the dreadful possibility of the special relationship between England and God ending in divorce. In the November the Fifth sermon, "The Church's Deliverances," Hooker asked why God's fury had so far spared "this little cottage, this little England" (p. 67). (Was Hooker the first Little Englander?) In the famous "farewell sermon," "The Danger of Desertion," which he preached, as is now clear, before the departure for Holland, Hooker pronounced the terrible verdict: "As sure as God is God, God is going from England" (p. 244).

Ten original documents are punctuated with essays by the four editors. George H. Williams writes on Hooker's early biography, Norman Pettit on "The Order of Salvation in Thomas Hooker's Thought," Winfried Herget on "The Transcription and Transmission of the Hooker Corpus," and Sargent Bush, Jr., on the Hooker canon. The volume deals competently with textual and theological problems and ends with an exhaustive bibliography of Hooker's published writings.

But one may regret that the editorial team did not include any historian competent to relate Hooker to what George H. Williams calls "the new understanding of early Stuart nonconformity" (p. 1, n.1). Williams and his collaborators betray little evidence of this understanding, and they have made no use of manuscript evidence. The impor-

tant correspondence of Samuel Collins of Braintree with Arthur Duck, Laud's chancellor, which tells us much about Hooker at Chelmsford, is cited only from the *Calendar of State Papers Domestic* and T. W. Davids's *Annals of Evangelical Nonconformity in Essex* (1863). And the account of John Rogers of Dedham (pp. 140-41) is taken from the *Dictionary of National Biography* almost word for word. There are mistakes, multiple mistakes in a series of comments on the Hampton Court Conference and related events (p. 359). Within one paragraph (p. 223) we are told that the Puritans "observed communion by preference every Sunday," that there was such an institution as "the Maundy Thursday Holy Communion of the Church of England," and that prophesying of the congregational type may have been a longstanding tradition "in the weekly rhythms of Puritan Chelmsford." Such statements suggest some detachment from the realities of English church life in the early seventeenth century. The consequence is a lack of informed comment where it is most needed—in connection with those writings that illuminate the gestation of Hooker's semiseparatist ecclesiology, *The Carnal Hypocrite* (1626), and the preface to William Ames's *A Fresh Suit Against Ceremonies* (ca. 1633).

PATRICK COLLINSON
University of Kent,
Canterbury

H. M. COLVIN *et al.* *The History of the King's Works*. Volume V, 1660-1782. London: Her Majesty's Stationery Office. 1977. Pp. xxiv, 535. £25.00.

The fifth volume of this series marks the thirteenth year of the enterprise, which now consists of two volumes on the Middle Ages, one on the period 1485-1660 (part one only has appeared) and a further volume on 1782-1851. The volume devoted to part two of the period 1485-1660 is in active preparation. No treatment of the modern period is planned, which is a shame since such a book would not only include new projects but would document changes in older ones—a crucial issue for historians of architecture and gardening. As a cutoff date 1851 does, however, make a certain amount of sense. It was in that year that the Office of the King's Works ceased to be a department of the royal household and became a government ministry. The switch was from an old order of royal patronage to a new one of parliamentary responsibility.

The present volume's dates are equally logical. The monarchy was restored in 1660 and continued to build in certain consistent ways until the reforms of 1782 (described in volume six) rather thoroughly remodeled the Office of the King's Works.

The dominant figure in the present volume is of course Christopher Wren, who was surveyor general for more than half a century. Wren was perhaps the last, perhaps even the only, major architect to hold this position. The volume deals *in extenso* with work at Winchester, Whitehall, St. James's Palace, Kensington Palace, and Hampton Court. It throws fresh light on these buildings and their grounds by using previously unpublished (and in some cases apparently unconsulted) documents. It provides, as the jacket claims, "the first coherent account of Wren's long tenure of office." Further dividends are the sections on Somerset House, Kent's projects for the Houses of Parliament, Buckingham House (later Buckingham Palace), Windsor Castle, the Tower, the Horse Guards, and No. 10 Downing Street.

The volume is divided into three sections to accommodate these essays—one on the history of the Office of the King's Works *per se* (mainly written by Colvin); one on the royal palaces, by Colvin and Newman with a contribution on Whitehall by Downes; and one, again by Colvin, on the public buildings. There are useful appendixes, for example on the royal gardens, and an index. The sixty-nine excellent plates illustrate plans, sections, and elevations of buildings and ceremonial occasions. The standard of scholarship is high.

G. L. HERSEY
Yale University

ERIC STOCKDALE. *A Study of Bedford Prison, 1660–1877*. Totowa, N.J.: Rowman and Littlefield. 1978. Pp. xii, 238. \$18.50.

Local officials administered most English prisons until the 1877 Prison Act transferred control of the gaols to the national government. Although prison authority changed hands, the earlier prison records remained in the local repositories and were generally forgotten until contemporary scholars realized that these records provided a unique source for local history. The latest example of this scholarly interest in local prison history is Eric Stockdale's study of Bedford Prison. Certainly, Bedford Prison merits a written history, because it played such an interesting role in English penology; John Bunyan wrote part of *Pilgrim's Progress* in Bedford prison, and in the late Georgian era such powerful families as the Whitbreads and the Russells were involved with the supervision of the prison. Most significantly, John Howard, as high sheriff of Bedford, began the modern prison reform movement after he saw the appalling conditions in the gaol.

Stockdale's approach is chronological. He begins with Bunyan's incarceration in 1660 and car-

ries the history through the passage of the 1877 Prison Act. The book is well researched and relies heavily upon unpublished court records in the Bedford County Record Office, supplemented by manuscripts from the Public Record Office as well as printed parliamentary reports. There is no bibliography, unfortunately, to indicate the diversity of these sources, but the extensive footnotes following each chapter do show that the book rests on ample archival research.

Stockdale's reasons for writing this book are twofold. First of all he hopes to refute the accepted view that local officials and gaolers ignored the gaols under their care. The author argues that the Bedfordshire officials spent considerable time and effort attending to prison administration and were genuinely concerned about the prisoners in their custody. Stockdale concludes that the prison was "never a forgotten backwater. The community was far more involved with the jail than is generally realized" (p. x). Stockdale's arguments are persuasive and well documented, and he makes a worthwhile contribution to English penal history by showing that the Bedfordshire officials took their gaol responsibilities seriously. Perhaps the two best chapters of the book deal with the efforts of Reverend Philip Hunt (1815–35) and present a strong case for including Reverend Hunt among the important early-nineteenth-century prison reformers. On the other hand, the chapter on John Howard is disappointing and tends to deal more with his parliamentary connections than with his efforts at the Bedford gaol.

The second purpose of the book is to "present a detailed picture of the prison at work" (p. ix). This rather vague aim goes unrealized, unfortunately, and a clear picture of prison life never emerges. This is partially because the author followed a strictly chronological approach so that matters vital to prison life are scattered throughout the book and are never treated as a whole. There is, moreover, little discussion of such topics as garnish, disciplinary problems, or prisoner self-government.

There are other problems with this volume. At times, it seems poorly organized and overwritten. For example, the second chapter is vaguely entitled "Some Contemporaries of Bunyan" and includes a desultory treatment of the arrest and committal process, benefit of clergy, escape attempts, and a collective biography of a local criminal family. Chapter four treats the subject of transportation but tends to ignore the subject and digresses into Bedfordshire politics. The author is also prone to lengthy quotes that encumber the narrative, when an apt paraphrase or summation would suffice. Page thirty-seven includes a typographical error listing the year 1837 rather than 1737.

These defects aside, it can be said that Stockdale has written a needed history of Bedford prison, which presents the much-maligned gaolers and sheriffs in a more favorable light. Readers should be cautioned, however, that this book delves deeply in Bedfordshire local history and can be heavy reading at times.

WAYNE J. SHEEHAN
Sul Ross State University

A. RUPERT HALL and LAURA TILLING, editors. *The Correspondence of Isaac Newton*. Volume 7, 1718-1727. New York: Cambridge University Press, for the Royal Society, 1977. Pp. xlv, 322. \$72.50.

Coincidentally published at the two hundred and fiftieth anniversary of Newton's death (1727), this seventh and last volume of his correspondence completes a monument to the current generation of Newton scholarship and, in particular, to its original editor, H. W. Turnbull, whose work was in turn continued by J. F. Scott and by the present editors, A. Rupert Hall and Laura Tilling. This last volume imitates its predecessors in accuracy—careful but not excessive explication of a text that is preceded by a brief but excellent introduction. It is extraordinary to realize that historians have waited so many centuries before gaining access to the complete correspondence of this most important of Western scientists.

Intense interest in Newton, his science, his religious beliefs, his personality, and, most recently, in the relation between his scientific achievements and the social context within which they were interpreted and explicated by his friends and contemporaries has made editions of his unpublished letters and papers imperative. Much of this interest has been generated by our postwar concern for the role of science in Western society, although that interest has not always been addressed by Newton scholars at the level of historical interpretation as such. At last the availability of Newton's complete correspondence should lead to an assessment of his own understanding of his political role and of the social meaning of his science. In that regard this final volume of correspondence may prove the most useful of all.

In the last phase of his life Newton received universal recognition as the scientific genius of his age. His creative powers had waned and his attention seemed consumed by public affairs. Many letters concern his obsessive desire to vindicate his mathematical originality, reiterated by his Continental followers, in the face of Leibniz's claim to priority. In this international correspondence Newton denied that the nature of the dispute was nationalist; to his close followers he described it

as "this national controversy" (p. 82). Clearly, Western science at its very origins was characterized by the kinds of ideological struggles, only of a national character, that now dominate relations between science in capitalist versus socialist or Communist societies.

Similarly in the early eighteenth century, just as in the modern world, ideologues operated on both sides, and this last volume of correspondence introduces the reader to a few of Newton's younger protégés—Henry Pemberton, Abraham de Moivre, James Stirling, Colin Maclaurin, Brook Taylor, and Jean-Theophile Desaguliers. Their letters are not personally very revealing, but they do bespeak a devotion to Newton that in some cases bordered on sycophancy. Such loyalty was seldom reciprocated with warmth. Throughout this correspondence Newton in old age remains true to the accepted portrait: efficient, cold, aloof, and slightly paranoid in responding to criticism.

Other characteristics are now also present. Newton was clearly immersed in his duties as warden and then master of the mint. He knew a great deal about the manufacture and value of capital, and he invested his own funds as well as those available to him at the mint in the South Sea scheme. The bursting of that bubble did not, it would seem, undo him. At his death Newton, the posthumous son of a yeoman, appears to have been worth about £32,000. His prosperity, given his origins and, as the letters reveal, a small army of impecunious relatives, parallels the achievement of other self-made men of his age, such as Locke, who also participated financially as well as intellectually in England's rise to power.

Like other liberal Anglicans of this post-revolutionary era Newton accepted the latitudinarian doctrine that charity should be given only to those willing to work. "Discretion in almsgiving," as Tenison called it, led Newton to serve on the board of trustees of Tenison's school for training boys in mathematics "as may the better qualify them to be put out apprentices." For a convicted counterfeiter, however, Newton, as master of the mint, prescribed his probable execution with the admonition "it is better to let him suffer."

This last volume of Newton's correspondence contains additions and corrections to the previous volumes. The Newton-Fatio relationship, still opaque, receives one or two amendments, although the editors missed an important memorandum by David Gregory (University Library, Edinburgh) revealing Newton's intense interest in Fatio's millenarian account of the prophecies of the French prophets. Inevitably, these oversights will occur in a correspondence (augmented by diary entries on Newton's conversation) that was geographically scattered and often recorded on

odd scraps of paper filed randomly. Now, also revealed, is Newton's real interest in his parliamentary candidacy of 1705. In short, specialists on the earlier period of Newton's life should consult this last volume for its minor alterations and additions to previous volumes.

The publication of Newton's complete correspondence makes more urgent the task of providing a new biography—one not focused solely upon his science and theology as might be expected. The integration of Newton's intellectual achievements with the social and political transformations that propelled Britain, by the first quarter of the eighteenth century, into a dominant role in European affairs will require a new breed of historian—one equally trained in the history of science and in general history. Should such a biography be produced by the next generation of Newton scholars, it will stand on the labors of historians like these who have given us this impeccable edition.

MARGARET C. JACOB

*Institute for Research in History and Baruch College,
City University of New York*

PETER DOUGLAS BROWN. *William Pitt, Earl of Chatham*. Boston: George Allen and Unwin. 1978. Pp. 448. \$29.75.

Peter Douglas Brown's biography of the colossus of mid-eighteenth-century Britain is a rewarding work. His vigorous account of Pitt's glorious war ministry, written with unquestioning faith in Pitt's military and strategic genius, reads easily. His narrative of politics, though denser and therefore less smooth, is well informed. In assessing the numerous figures whose lives crossed Chatham's, Brown passes judgments that—with the exception of his verdict on Temple—are fair-minded and even generous. Of greatest importance to historians, however, is his use of the Chatsworth MSS to redefine and enhance the importance of the duke of Devonshire. This shadowy figure, scarcely known to posterity despite a brief tenure at the treasury, emerges as a major mover in mid-century politics. In sum, Brown has given us a useful account of Chatham's astonishing career.

But the study is not without defects. The description of the Asiento is inaccurate, and the account of Wilkes's contest with the ministry in 1768–69 is misleading. About the nature of Chatham's recurring "gout" Brown is oddly uninquisitive, while all that he wishes to tell us about the strange affliction that destroyed Chatham's command of his ministry in the late 1760s is that his mind suffered a "relapse" in 1767 and "began to heal" in 1769 (pp. 408, 357). Similarly disappointing is Brown's failure to examine Chatham's

speaking talent, even though it was his rhetoric that gave him his initial celebrity and later allowed him to retain influence even in ill health and hopeless opposition. A final jarring omission is the framework of political and constitutional ideas within which eighteenth-century men of affairs operated.

My final criticisms are of a different order from the foregoing ones. They are meant to suggest that Brown's conception of biography itself is not as rich as it might be. He has given us in effect an external biography. That is, although he tells us what Chatham did, he only rarely tells us why he did it. We may perhaps attribute this reticence to a reluctance to push beyond what the evidence suggests about Pitt's internal life, and such caution is often admirable. But no such reluctance characterizes Brown's approach to external events. Indeed, he gives no indication at all that he is troubled by the evidential ambiguities that underlie the historiographical controversies of the period. His reluctance to make the uncertain coherent is therefore selective. Brown offers no explanation, for example, for Pitt's extraordinary political behavior in 1744–46 and early 1766. In each instance the Great Commoner acted in fashions that invite being called irrational. A biographer who is not interested in even trying to understand such behavior is, I think, disabling himself and impairing his work. The result, in this instance, is a valuable book that nevertheless falls short of distinction.

My nomination for the most picturesque typographical error of the year comes from page 49, where we find Frederick the Great playing the "lute."

REED BROWNING
Kenyon College

JACK SIMMONS. *The Railway in England and Wales, 1830–1914*. Volume 1, *The System and its Working*. Leicester: Leicester University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1978. Pp. 295. \$24.00.

Jack Simmons is England's foremost authority on his country's nineteenth-century railways. His publications and joint editorship of the *Journal of Transport History* from its founding in 1953 to 1973 have enabled him to shape our understanding "of the years in which the railway achieved and maintained its dominance in the transport system" (p. 16). In addition, the Transport History Collection of over twenty-thousand items, which he gathered at the University Library at Leicester, represents an important body of research materials on every form of transport.

With this extensive background and these kinds

of resources, Simmons has begun a four-volume history of the railways of England and Wales between 1830 and 1914. He proposes to tell "not what the railway was, but what it did" (p. 15). He is not so much interested in delineating the details of construction or the way in which passengers and goods were carried; rather, he seeks to mark out the railway's impact on society.

This initial volume is admittedly a sketch, the foundation on which Simmons will build the remaining three volumes. Its first four chapters describe the physical expansion of the railway system. Much of this material is well known, but Simmons has given it skillful organization and synthesis. He discusses the lack of planning that characterized this growth, the competition and proliferation that frequently resulted in double service, the advantages of railways to towns for industrial development and to landowners for increased value of property, and the speculation and fraud that occurred. The year 1876 was a turning point, after which the suburban railways complemented the older lines. By the turn of the century, new kinds of land transport began to intrude on the railways' freight-carrying and passenger business. Simmons treats greater London as a separate entity; here a deliberately planned, integrated system emerged, with emphasis on the underground and the widespread substitution of electricity for steam.

Each of the remaining five chapters provides background for more penetrating analyses that are promised in the subsequent volumes. Using numerous illustrations, Simmons demonstrates the interrelationships in engineering between roads and canals on the one hand and railways on the other. Although railways were built on a larger and more complex scale than either of the earlier forms, they used much of the knowledge and many techniques that had previously been developed. Cheap labor and local materials helped to keep construction costs to a minimum, but the engineers did not sacrifice quality for low cost. Simmons holds that no serious accident occurred up to 1913 that "could be attributed to any failure in the permanent way" (p. 151). English engineers often designed, built, and repaired their locomotives in their own shops, incorporating certain improvements such as compounding and the bogie to give faster and safer service. Although this practice seemed to serve well, it tended to induce a sense of complacency, a feeling that Britain's way was superior without having to "shop around" for the best that had been developed elsewhere. Yet the British railways satisfied the demands placed upon them, and they contributed much to that international advantage that Britain enjoyed between 1870 and 1914.

Except for a mild reservation about the role assigned to the Liverpool and Manchester (it was even more significant in the history of British railway development than Simmons allows), this volume, with its excellent end map, has fulfilled its purposes. The remaining three volumes in this study are eagerly awaited.

ROBERT E. CARLSON
West Chester State College

D. A. REEDER, editor. *Urban Education in the Nineteenth Century*. New York: St. Martin's Press. 1978. Pp. ix, 144. \$17.95.

Although urban history has enjoyed a vogue in recent years that has attracted the attention of diverse specialists, one facet of the subject that has received less attention from professional historians than its importance merits is urban education. D. A. Reeder, in editing for publication the proceedings of the 1976 conference of the History of Education Society of Great Britain, has attempted to focus wider attention upon this need. His volume includes an introductory essay, four papers delivered at the conference but modified for publication, and two additional papers. The essays, with a single exception, concern problems of urban education in Great Britain, largely during the nineteenth century. The exception is a historiographical essay commenting upon recent American work in the field, written by Sol Cohen of the University of California at Los Angeles. Reeder's introductory essay serves essentially the same function for the state of scholarly work on urban education in Great Britain as Cohen's does for the United States.

The authors of the British articles include two lecturers in education and one each in the history of education, sociology, and history at the Universities of Leicester, Bradford, Leeds, and Liverpool. Reeder was for a decade Head of Education at Garnet College of Education and editor of *The Vocational Aspect of Education*. He is an urban historian by training and avocation.

Both Reeder and Cohen propose a variety of methodological approaches as avenues of investigation; and a number of them are illustrated by the essays in this volume. D. Fraser, for example, in his "Education and Urban Politics c. 1832-1885," relies on his own research in Victorian urban politics and that of others in politics, education, religion, and the formation of public opinion to relate local to national problems on a comparative basis. W. B. Stephens, in his "Illiteracy and Schooling in the Provincial Towns, 1640-1870: A Comparative Approach," draws upon his experience in local history, which includes several years

as editor of the *Victoria County History* volumes. He portrays the variety of towns found in provincial England from the seventeenth century and reveals the educational impact of urbanization.

W. E. Marsden's "Education and the Social Geography of Nineteenth Century Towns and Cities" examines the forces that affected the distribution of schools within and between towns and cities in the latter part of the century. As a historical geographer, he has developed the study of "socio-spatial" aspects of urban processes and employs it to show how changes in the internal organization of urban districts affected schooling. Reeder's conference paper, "Predicaments of City Children: Late-Victorian and Edwardian Perspectives on Education and Urban Society," examines the cultural perceptions and social anxieties of middle-class city-dwellers who aspired to shape the direction of urban development.

Photographs, charts, graphs, and maps are included, as well as a select reading list. The essays contain copious bibliographical references. On balance, the volume provides useful insight into recent professional historiography of urban education on both sides of the Atlantic.

ROBERT G. MCPHERSON
University of Georgia

CHRISTOPHER KENT. *Brains and Numbers: Elitism, Comtism, and Democracy in Mid-Victorian England*. Buffalo, N.Y.: Toronto University Press. 1978. Pp. xiv, 212. \$17.50.

This book's origin in a doctoral dissertation probably explains why its structure is that of a disconnected fragment of intellectual history, but the fragment has in itself such consistency and interest that the missing connections with a larger whole can either be readily supplied by the specialist or simply ignored by the generalist. *Brains and Numbers* is an account of a small group of university men (termed "radicals" by Christopher Kent), mostly from Oxford, who tried to come to grips with the rights and needs of the masses in the emerging democracy of mid-Victorian England. The author succeeds in presenting a convincing account of the background, motivation, limitations, and successes of this group.

The group in question consisted of men who finished their academic training in the 1840s and 1850s—Frederic Harrison, John Morley, E. S. Beesly, J. H. Bridges, Leslie Stephen, Godfrey Lushington, James Bryce, A. V. Dicey, and others. They shared a common educational background and social environment, and they shared a common concern about the responsibilities of an intellectual elite in an increasingly industrialized

and democratic society. They were, and were happy to be, part of the mid-Victorian establishment. They were in no sense alienated intellectuals; rather, they were searchers for a role that intellectuals like themselves might play to resolve more effectively England's social and political problems.

In Kent's estimation the thought and influence of Samuel Taylor Coleridge, Thomas Arnold, F. D. Maurice, and even Thomas Carlyle structured the initial routes that these "radicals" explored in their search. Aspects of the intellectual system of Auguste Comte strongly influenced most of them later. The very movement of the 1840s and 1850s for university reform at Oxford was led by followers of Coleridge and Arnold who wanted to nationalize the universities in order to force these institutions to enter the mainstream of English life. Intellectual talent rather than social class was to determine who went to the universities, and the elite that emerged from these schools would command the respect and following of all classes in the nation. So, at least, went the logic of the university "radicals." In the 1850s and 1860s, when these men took their cause to groups and representatives of the lower classes, they seemed to be accepted and understood. The Reform Act of 1867 made it possible for many of these same lower-class individuals to express themselves with a vote. When a number of the university "radicals" stood for parliamentary seats as popular candidates in 1868, however, they were defeated.

The middle section of Kent's book describes the Comtist movement in England during the 1850s and 1860s, and shows how Comte's thought, or aspects of it at least, provided a systematic framework through which these university "radicals" could understand and extend the role of a middle-class intellectual elite. "It not only legitimized their pretensions to intellectual authority but also provided a powerful ideological solvent for social irritants to those who were ready to entertain fairly radical notions of social change for essentially conservative ends—a sort of prospective conservatism rather than the more common retrospective variety. Comtism met the anxieties of classically educated, amateur-oriented intellectuals by providing a scientifically sanctioned blueprint of the future which assured their continued relevance despite the increasing industrialization, specialization, and democratization of society" (p. xiii). The book's final section treats in detail the careers of Frederic Harrison and John Morley, both greatly influenced by Comte, and their attempts to solve their dilemma as intellectuals without an established niche through journalism and politics.

This volume has a bibliography and an adequate index, and in general is well produced. It

has, however, an unacceptable number of simple printing mistakes and typographical errors, so that one might legitimately wonder whether it was ever finally proofread.

LAWRENCE F. BARMANN
St. Louis University

M. JEANNE PETERSON. *The Medical Profession in Mid-Victorian London*. Berkeley and Los Angeles: University of California Press. 1978. Pp. x, 406. \$17.50.

M. Jeanne Peterson's well-written "sociological approach" to *The Medical Profession in Mid-Victorian London* is a most useful contribution to British social history. The book benefits from a number of recent studies of sociologists as well as from W. J. Reader's historical investigation of *Professional Men*. Medicine in early nineteenth-century England had a difficult time differentiating itself both from the craft of surgery and the apothecary's commerce in drugs. This volume describes this process of self-realization, taking the reader from 1858, when the profession was legally defined, until 1886, when parliament established the institutional basis for its modern structure. From a variety of sources, Peterson has uncovered the social origins of the Victorian doctors, their education, the *cursus honorum* of the profession, the life styles and incomes of the medical men of the capital, as well as the internal dynamics of this nineteenth-century guild.

Doctors in London, Peterson argues, were sharply divided between a gentlemanly elite and a mass of hard-pressed general practitioners. The elite, usually educated at Oxford and Cambridge before beginning a medical apprenticeship and associated with one of the teaching hospitals, enjoyed the income and the life style of gentlemen, attended the royal family and the peerage, and, at the top, were rewarded by knighthoods. One of them, Joseph Lister, even became a baron. By patronage and, frequently, nepotism they recruited their successors. Far different was the condition of the general practitioner, desperately striving for the status of gentleman, indeed, for economic survival, and regarding the members of the elite as his social and professional adversaries. Peterson makes good use of literary sources, notably Dr. Conan Doyle's *The Stark Munro Letters*, for her picture of the predicament of the poor doctor. She writes perceptively of the medical "entrepreneurs" of the time, frequently charlatans specializing in cures for sexual malfunctions, and of a more worthy number who sought to bypass the elite's hold on the teaching hospitals by creating new institutions for the treatment of special dis-

eases. The elite strove, with relative success, to regulate the first group and co-opt the second.

This study is largely concerned with the growth in the social and professional prestige of doctors. Despite the increase in the authority of science, the profession continued to be beset by challenges to its authority, not only by the lay boards of governors of the great hospitals but even by nurses, who sometimes benefited from the support of persons of high social status. Using all of the traditional devices of the guild, not the least of which was the exclusion of others from its secrets, and taking advantage of the growing recognition of the social value of their expertise, doctors were at last able to become masters of their own house. Soon, indeed, as we know, they emerged as virtually a lay priesthood.

Peterson suggests in an all-too-brief conclusion that the National Health Insurance Act of 1911 helped to free the general practitioner from his financial difficulties. She makes the case that medicine was a model for the development of the other learned callings. Its virtual incorporation into the state bureaucracy in the late 1940s may well pre-empt their fates as well.

BERNARD SEMMEL
State University of New York,
Stony Brook

ANGUS MACLAREN. *Birth Control in Nineteenth-Century England*. New York: Holmes and Meier. 1978. Pp. 263. \$16.50.

The dramatic decline of the birth rate in Victorian England from the 1870s has been considered a crucial factor in social change. Measured and evaluated by demographers and historically minded sociologists, most studies have been largely statistical and discussed in terms of national trends. Interpretive works are few, and these center on the fecundity of women of all classes, the demand for labor within the working classes, and contraceptive practices of the upper and middle classes for mainly masculine socioeconomic purposes. Regarding contraception, the popular thesis is that knowledge and use of mechanical devices filtered slowly down the social scale. Angus McLaren does not challenge the statistics but scores the insufficiencies of these studies, because they do not "tell us the why and how." They wrongly assume "the embourgeoisement of the working classes," and they focus on a unilinear model of the progressive adoption of contraception. They also avoid analysis of the nineteenth-century birth-control debate that involved cultural confrontations: "the struggle of middle-class propagandists [of both Right and Left] seeking to manipulate working

class attitudes . . . for political purposes, and . . . the clash of differing attitudes of men and women" (p. 12).

Relying mainly upon primary sources and eschewing gross deterministic and ideological generalizations (despite an unmistakable Marxist and feminist perspective), Angus MacLaren analyzes the public controversy and its ramifications for private fertility control. He underscores the limitations of statistical-sociological approaches to understanding Victorian attitudes, behavior, and the results of birth-control activity by showing that abortions and miscarriages went uncounted and contraceptive techniques often did not work. MacLaren finds that birth-control practices were sometimes employed for spacing births rather than for controlling family size. He argues that methods of prevention hinged on the relationship of the sexes and on attitudes toward sexuality. More women than we have been led to expect appear to have attempted to control their fertility; social, moral, and political forces made birth control a debatable public issue and an often extremely private (though sometimes "unthinkable") practice.

MacLaren organizes his study around four main themes: (1) there was never any "pure ideology" for or against birth control, (2) knowledge and means of contraception had been available from 1800 onward so that diffusion of information was less important than adjustment, especially among the working classes, to new socioeconomic conditions, (3) the role of women in birth-control practice "has been strangely played down" (p. 14), and (4) birth control was widely considered a form of medical "self-help" and often did not involve mechanical devices. Using contemporary sources innovatively MacLaren handles medical and eugenic literature as well as the writings of Malthusians, neo-Malthusians, pornographers, journalists, quacks, commercial advertizers, and midwives. The correspondence of less reticent Victorians of both sexes supports his contention that the idea and practice of birth control differed from class to class and sex to sex. MacLaren's study (thirteen loosely linked chapters) is admirably probing but not definitive. The author does not consider, for example, the question of the variations in fertility control and family size from region to region; nor does he pursue analytically the crucial area of pervasive Victorian mentalities (and psychological conflicts?) pertaining to contraception. But this book is indispensable for introducing the subject in historical perspective, and it is the essential starting point for future scholarly investigation.

BARBARA KANNER
Occidental College

GERALD L. GEISON. *Michael Foster and the Cambridge School of Physiology: The Scientific Enterprise in Late*

Victorian Society. Princeton: Princeton University Press. 1978. Pp. xix, 401. \$27.50.

The last three decades of the nineteenth century constitute a curiously obscure period in the historiography of science, at least compared with the first two-thirds of that century or with the first third of the twentieth. In large part this may be due to the fact that this period seems to be a transitional trough between the towering scientific achievements of mid-century—of Charles Darwin, Claude Bernard, and Hermann Helmholtz, for example—and the revolutionary, early twentieth-century scientific breakthroughs. George Sarton once dismissed the Renaissance vis-à-vis the history of science in similar fashion as a trough between the great medieval achievement and the mighty ones to come of the Scientific Revolution. Scholarship over the past twenty-five years has shown that this Renaissance "trough" was in fact an intensely active period of science in its own right and a period preparative of what was to come. Such is obviously the case with the late nineteenth century as well.

In this richly textured book Gerald L. Geison examines one major development in English science in the late nineteenth century: the rise of a school of physiological research at Cambridge University under the tutelage of Michael Foster. England is of particular interest in this period, for it was then that science became significantly incorporated in the two ancient seats of higher learning, Oxford and Cambridge. Especially with the foundations at Cambridge of the Cavendish Laboratory and the school of physiology associated with Foster—both in the 1870s—England was able to develop research centers that could rival the great centers of Germany in these fields.

In his study of the Cambridge school of physiology, Geison examines the background of physiological research and teaching in England in mid-century (poor to nonexistent); the education and early career of Foster at London University, where he came into contact with the influential T. H. Huxley; his acceptance of a praelectorship at Trinity College Cambridge in 1870; his success there in generating interest and momentum for the teaching of physiology; and his gathering about him a group of devoted research students to build the "school." This book, then, is about the *process of coming into being* of Foster's school; indeed, Geison's story is all but ended at just the time that Foster himself begins to win those tangible marks of real achievement, such as his professorship at Cambridge in 1883.

Lest this sound like "great men of science" history, I hasten to advise that it is not. Geison sees, indeed, an intriguing paradox at its core, namely, that Foster was not a very great research scientist.

A principal intent of Geison is to unravel "the hitherto mysterious process by which a 'second-rate' scientist came to found an undeniably great research school." The process, as Geison sees it, was neither obvious nor simple; it depended as much on personal imponderables of Foster, such as his charra and political savvy, and on people of power and influence whom he convinced to aid him, as it did on his tangible abilities in research or excellence in teaching. Yet Geison does not discount the research carried on by Foster and his immediate pupils; recognizing that a necessary prerequisite to a "research school" is a guiding "style" of research—the type of problems taken up, the experimental strategy employed, the theoretical orientation—Geison attempts to show that perhaps the critical element of the success of the Cambridge school was that Foster did, initially, do some research and that the problems he was investigating were interesting and important ones.

The last third of the book is devoted to this Fosterian research program: the study of what causes the rhythmic expansion and contraction of the hearts of animals. Foster had been interested in the problem before he came to Cambridge, and he had always held a heterodoxical viewpoint, at least with regard to the dominant position of German physiologists. At Cambridge he set his first research students to work on this problem, and, for roughly the first decade of the Cambridge school's existence, climaxing with the epochal paper of Foster's student, Walter Holbrook Gaskell in 1883, this problem constituted the core of the Cambridge research program.

It was also the year 1883 in which Foster gained his professorship at Cambridge. By then he had abandoned personal involvement in research or even much teaching for a career as an administrator, an editor, and a power broker in British science (as biological secretary of the Royal Society of London). Yet the Cambridge school continued to grow in renown. In a concluding section Geison attempts to come to grips with the sustained growth of the school. Ironically, one positive factor in Geison's view was Foster's own abjuration of active research; this kept him from exercising what might have been a constricting influence on the burgeoning multidirectional research of his best students. Geison gives some discussion of the nature of that research and enumerates the researchers and research students who came to Cambridge during the rest of the century. But he does not say much about one component of the development of the school: the process by which the foreign reputation of the school—slight under Foster in the 1870s—was enhanced thereafter so that foreign, even German, researchers were coming in the 1890s. Regarding Foster himself, more consideration might have been given to the role of his

Textbook of Physiology and Studies from the Physiological Laboratory of Cambridge University in the spread of his own reputation and that of his school.

There is one limitation in the actual format of the book: its lack of a bibliography. While not a serious drawback (one can find all the references in the footnotes helpfully placed at the bottom of each page), it is nevertheless an inconvenience. But these minor criticisms are not meant to distract from my appreciation of a really rich, illuminating, and important book.

SEYMOUR MAUSKOPF
Duke University

MICHAEL BAKER. *The Rise of the Victorian Actor*. London: Croom Helm. Pp. 249. \$14.50.

Acting, as Noel Coward warned Mrs. Worthington, is a singularly precarious occupation, yet Michael Baker argues convincingly for a considerable improvement in the actor's status and income between 1830 and 1890 as part of the more general process of modernization of the English theater. Although a handful of stars were lionized, and there was a growing popular audience for the stage, the early Victorian actor was a cultural vagrant whose craft was dismissed by respectable opinion as vulgar, frivolous, and fundamentally immoral. The common player was in economic terms a casual laborer, sustaining his livelihood through the traditional resources of family connection and membership of an arcane and supportive occupational subculture. Yet by the 1870s, the actor (and the actress, to whom Baker devotes a separate chapter) had broken out of social quarantine and economic marginality and become a professional celebrity in a new world of expanding "middle-brow" culture. Not only the stars but the company actors came to enjoy better pay, greater security, less demanding working conditions, and a much improved social image, though Baker acknowledges how little such advances benefited the majority of the rank and file and points out that the concern for respectability bred an artistic conservatism in writing and performance that reinforced caste distinctions in the theater and reduced opportunities for lower-class entrants. Moreover, the striving for status impeded the development of a coherent professional and educational structure for stage careers. The changes for the better were nonetheless remarkable.

The transformation is attributed to an influx of middle-class newcomers determined to raise standards parallel to those of their peers in other emergent professions. Baker also points to the erosion of clerical sanctions against the stage amid a new climate of intellectual agnosticism and the expansion of leisure and communication facilities that

stimulated the demand for diversion dignified as "culture."

In such a relatively slim volume, the arguments must be taken as suggestive rather than conclusive, but there is much of substance here on the inner mechanics of the profession and an abundance of instructive asides. Baker has not moved far from familiar sources, but he has exploited these with imagination and has made a valuable contribution to the pioneer social history of the Victorian theater. Acting, he suggests, is a freakish and, in this period, increasingly elitist profession; but he provides a useful model for other occupational histories, and it is through such studies, particularly of marginal and less celebrated groups, that we can illuminate the growing significance of leisure and the service sector for the social history of the Victorian years.

PETER BAILEY
University of Manitoba

GILBERT HERBERT. *Pioneers of Prefabrication: The British Contribution in the Nineteenth Century*. (The Johns Hopkins Studies in Nineteenth-Century Architecture.) Baltimore: Johns Hopkins University Press. 1978. Pp. xii, 228. \$17.50.

Throughout much of the former British Empire the corrugated-iron chapel still bears witness to a nineteenth century in which colonial settlement was sustained by industrialization at home. Though increasingly interested in the more functional aspects of Victorian building, British architectural historians have almost entirely ignored prefabricated and temporary constructions. With the exception of the Crystal Palace and a few other feats of obvious technical virtuosity, these have always passed as inferior work when viewed from the heart of the empire. Gilbert Herbert's interest seems to have arisen during residence in Australia, where architectural historians, surrounded by the cast-iron balconies, verandahs, and canopies, which were the sole adornment of so many buildings, cannot permit themselves the luxury of such a distinction. After further work in South Africa and Britain and world-wide correspondence, he now presents the first major study of British prefabricated design and construction during the nineteenth century.

The main development of the period was from wood to iron as the relative cost of the latter fell. Herbert traces from as early as the 1770s the growth of an industry that produced wooden cottages, which emigrants could take to the colonies, notably Australia. From the 1840s manufacturers turned increasingly to iron, producing a much wider range of buildings (including factories and

lighthouses) with more ambitious designs. It was perhaps the iron church that brought out the best in both architects and artificers, with major structural elements and decorative features in cast iron and extensive use of corrugated iron for walls and roofs. There was also a sturdy strain of cast-iron villas and hotels in which the material was allowed to demonstrate its qualities far more freely than ever in Britain.

This most neglected of British export industries was nevertheless a precarious one. Home-produced wood and iron designs never entirely solved the problems posed by hot and humid climates. As the colonial economies matured they generated more demand for stone and brick structures. The residual need for prefabricated buildings and components was met increasingly by local manufacturers. Despite considerable technical advances, British building, with the exception of industrial and transport structures, remained largely impervious to prefabrication. Home demand was thus too weak and fluctuating to generate a lasting tradition. The lessons of the past were often forgotten as when, in the Boer war, no attempt was made to resurrect the perfectly practicable system of prefabricated hospitals developed by Brunel for the Crimea. Meanwhile, the huge American market was completely lost to manufacturers in the United States.

Materials have also been a problem for the author. The manufacturers left few records and many of their products have disappeared. Herbert has had to rely for the most part on broad trawls through the Victorian architectural and engineering press, supplemented by site visits and local research. The resulting coverage is inevitably uneven but the author does well to distinguish some general trends and developments. The main strength of the book lies, however, in the detailed expositions of individual buildings and projects, which, backed by copious illustrations, are fully rescued from an undeserved obscurity.

ANTHONY SUTCLIFFE
University of Sheffield

D. R. SARDESAI. *British Trade and Expansion in South-east Asia, 1830-1914*. New Delhi: Allied Publishers Private. 1977. Pp. viii, 302. Rs. 60.

NICHOLAS TARLING. *Sulu and Sabah: A Study of British Policy towards the Philippines and North Borneo from the Late Eighteenth Century*. New York: Oxford University Press. 1978. Pp. 385. \$39.00.

D. R. Sardesai is a rising authority on Southeast Asia, and his new book makes a valuable further contribution. It draws on a wide range of archival

materials to analyze the course of British expansion before and during the "new imperialism" of the later nineteenth century. Britain's two most active competitors, the French and the Dutch, come into the story. (There is an odd slip on page 32 where the Stadholder William of Orange in 1787 is referred to as the "Dutch King.") American enterprise figures too; the author points out that the opening of the Suez Canal, which helped European exports to reach the region, coincided with the transcontinental railways, which did the same for American goods. Sardesai is interested in the general problem of imperialism in that epoch, and his book has a very thorough bibliographical equipment that will make it a useful work of reference on the subject. He dismisses Hobson's "myth" of a free trade interlude when Britain felt no expansionist urges (p. 7). He is critical too of the Hobson-Lenin emphasis on capital export as the key to the revived or intensified empire-building of the years after 1875. On the other hand he finds the noneconomic explanations that have been offered quite inadequate, at least as regards Southeast Asia. Concentrating on trade rather than investment, he argues that the gains accruing from political control in this region were large, though how large is, he admits, impossible to calculate. They were secured, moreover, at no expense to Britain, since the costs were borne by taxpayers in India and in the newly acquired territories (pp. 275-77).

Burma and Siam with Malaya, are the main concerns: the first destined to be swallowed up, Siam to survive as a buffer state shorn of its dependencies in the peninsula. Britain claimed a special status, or "paramountcy," with the right to exclude any foreign political influence, in all areas considered to affect the security of India. Ministers in London were as a rule averse to adventures, but there was a chronic tug of war between their policies of restraint and the ambitions of colonial officials and traders. Swettenham emerges as an aggressive expansionist in Malaya, Rajah Brooke as a "swash-buckling" bully (p. 87). In 1885 the government was maneuvered into annexing Upper Burma by mercantile pressure, playing on rumors of French meddling, most of which were "deliberately fabricated by British commercial interests" (p. 201). A leading part in the agitation was played by the Rangoon Chamber of Commerce; and we hear much about the chief instigator, the Bombay Burmah Trading Corporation, whose lucrative teak forest concessions were at stake. Meanwhile in Siam Britain was achieving such a privileged position that there was no need of forcible intervention. Malaya was a different matter. For a long time London followed a consistent line of not meddling with Siam's rather shadowy rights in the

north—a pallid oriental equivalent of paramountcy—for fear of undermining Siam and laying it open to French penetration. Traders and officials at Singapore clamored for action, on the ground of Malaya's anarchical condition, and they could push their case through the Colonial Office as against the Foreign Office. In the southern principalities they were getting their way after 1873; in 1909 the northern States were transferred from Siamese to British suzerainty.

Nicholas Tarling's massive study has a far narrower focus. Sulu was a loose feudal-piratical sultanate, based on a group of islands to the south of the Philippines, with hazy claims stretching southward into Sabah, or northern Borneo. These claims stirred up triangular disputes, running through the nineteenth century, among Spaniards, Dutch, and British. During the 1880s, with the ramshackle kingdom of Brunei crumbling, London after long hesitations came to be willing to avow a sort of ill-defined protectorate over Sarawak and the area secured by the British North Borneo Company. Americans were on the scene as traders or adventurers, and then more prominently as a result of the annexation of the Philippines in 1898. All these complexities made Sulu and Sabah a kind of Schleswig-Holstein of the Far East. It may be objected that this study of them is too minutely detailed. But microscopic scrutiny of some corners of history can help toward a realistic understanding of how history has been made. Tarling marshals his host of details (chapter four has 375 footnotes) with easy mastery, and very few visible inaccuracies: Isabel II for instance did not die in 1868 (p. 118) but was dethroned. And the record opens up wider vistas in a number of directions. It illuminates the workings of diplomacy and of British officialdom and the dubious origins of the Borneo Company out of concessions to Overbeck—an Austrian baron—and the Dent brothers. Illustrations as well as maps embellish a handsomely produced volume.

V. G. KIERNAN
University of Edinburgh

H. C. G. MATTHEW, editor. *The Gladstone Diaries*. Volume 5, 1855-1860; volume 6, 1861-1868. Oxford: Clarendon Press. 1978. Pp. lxxii, 545; 706. \$92.00 the set.

"Will he never die?" H. C. G. Matthew reports the question is often asked by his family about the author of the diaries he is in the midst of editing. Volumes five and six of the diaries of William Gladstone, a record of the years 1855 to 1868, carry the chronicle of his life well beyond its mid-point. Nevertheless, fifteen further manuscript volumes

remain to be published. The Matthew family will have to be patient. Of Matthew's own patience, these volumes, like their predecessors, provide unquestionable evidence. Throughout the volumes we are assisted in the unraveling of cryptic references by the editor's willingness to dig on our behalf; Matthew has traced obscure tracts and sermons and has identified less-than-eminent personages. And the raw material of the diary as a whole is refined in a perceptive introductory essay.

Gladstone, as has often been remarked, was no Greville. The entries are almost always short, often little more than a list of letters written and books read. Yet studied in stretches of a year or so, the diary tells a good deal about the man and his times. These volumes contain none of the sensational revelations that made the publication of their immediate predecessors a literary event; the mark signifying flagellation occurs only once. The countless references to rescue work among prostitutes still appear, but they are, as the editor correctly remarks, no more than formulaic. Self-criticism and self-doubt have become ritual as well, whose very repetitiveness, readers must constantly remind themselves, had particular meaning for Gladstone. Occasionally, the heavy prose of these passages parts, and, in a sentence or phrase, we are suddenly in tune with the mind of the man who wrote it. Matthew concludes his introduction with one such entry, written in 1864, when Gladstone's political future was particularly uncertain. Lamenting the death of the Duke of Newcastle, he writes that it "removes the very last of those contemporaries who were also my political friends. How it speaks to me! 'Be doing; and be done'" (6: 308).

These years, when Gladstone could not be certain whether or not his apparently anomalous position as a Peelite relic meant that he was in fact "done" politically, were nevertheless filled with doing. His term as chancellor of the exchequer was notable as an illustration of the way in which government was coming to mean policy-making as well as administration. We discover Gladstone turning free trade and balanced budgets into articles of faith, thereby shaping the Whig-Liberal alliance in such a way as to ensure his eventual position as its leader. Political doing was not only policy-making but politicking. These were the years when Gladstone deserted his Oxford constituency for "the people." The diary shows him fascinated with popular politics: "Went, by a desperate push, to see Garibaldi welcomed at the Opera," he reported in 1864. "It was good, but not like the people" (6: 269). Later that same year he spoke at Bolton: "Addresses presented me at the Temperance Hall in a dense assembly and stifling atmosphere: replied in a speech. . . . I was much struck with the people" (6: 306).

Much of Gladstone's time during this middle period of his life was spent apart from politics. At home he pursued his interest in classical scholarship. He immersed himself in domestic affairs to a degree that belies the stereotype of the Victorian male, busying himself with his china collection, packing for trips abroad, and spending long hours teaching his children. By this time the country house weekend was an institution and Gladstone a frequent host and guest: "It is always rather heavy here [at Eaton Hall, seat of the Duke of Westminster]. The hosts seem bored with their work and anxious to end it" (6: 325). Gladstone preferred Cliveden with the dowager duchess of Sutherland, one of the several women whose company was apparently a necessary supplement to that provided by those of his immediate family.

The "doing" most frequently recorded in the diary was writing and reading. Gladstone's list of correspondents is immense. Wherever he was, however much he may have protested to himself that he should "be done," he kept in touch with powerful men and important ideas. Matthew intends eventually to publish a list of all the books listed as read in the diary. This fascinating compilation will show that Gladstone, even when most determined to leave politics, could not separate himself from the world of affairs any more than he could from those men who, like himself, directed them. The diary is, above all, the journal of a man of the Victorian governing class. It was a class that had reshaped itself in the early nineteenth century to admit the commercial likes of William Gladstone, and that Gladstone, in turn, was reshaping to admit "the people." In both instances, accommodation was on terms more advantageous to those of the old order than to those of the new. Gladstone, as he records his life in London, at country houses, and in the industrial provinces, unconsciously describes that continuing process of accommodation, one that the "doers" expected would enable them to continue to exercise power—never to "be done."

STANDISH MEACHAM
University of Texas,
Austin

PAUL MEIER. *William Morris: The Marxist Dreamer*. In two volumes. Translated by FRANK GRUBB. Atlantic Highlands, N.J.: Humanities Press. 1978. Pp. xiv, 287; 289-597, lxx. \$57.50.

Paul Meier's massive study of William Morris first appeared in France in 1972. This excellent translation makes available to English readers the most detailed examination of the socialist vision of Morris as well as the sources and influences out of

which it developed. Volume one presents an extensive treatment of the various strands of thought that influenced or may have influenced Morris's mature outlook. The second volume provides a close analysis of the utopian vision itself, not only as it appears in *News from Nowhere* but in the letters, talks, and newspaper pieces of Morris.

Meier reinforces the view, advanced by E. P. Thompson in his major study in 1955 and recently republished in a revised edition, that Marxist ideas had a decisive impact on Morris. Like Thompson, the French scholar is convinced that the utopian writings of Morris are not only fully compatible with the Marxist system of thought but represent a significant extension and enrichment. Yet, in their interpretations of Morris's relationship to Marxist thought, Meier and Thompson diverge in significant ways. For Thompson, most clearly in his revised study, Morris adopted Marxism because it confirmed and deepened his own romantic critique of capitalist society and enabled him to break out of the impasse into which his essentially middle-class and reformist outlook had led. Although Meier pays considerable attention to romantic influences in preparing Morris for socialism, he views his relationship to Marxist ideas as a "conversion." He is occupied mainly with the task of demonstrating the close correspondence between the ideas of Morris and those of Marx and Engels.

Central to Meier's argument is the claim that Morris possessed a clear grasp of Marx's notion of the "two stages"—the notion that the communist society would only be attained after an initial socialist phase. But since Morris employed the idea of two stages well before the first publication of Marx's notion, in the posthumous "Critique of the Gotha Program," Meier speculates that Morris must have picked up the idea from conversations with Friedrich Engels. Similar conjectures are employed from time to time to establish links between Marxist theory and the communist vision of Morris. They reflect a rather mechanical approach to Morris's intellectual development that contrasts with Thompson's reading, which is more nuanced and sensitive. It is, to some extent, the contrast between a French Marxist, viewing Morris from a distance, and an English Marxist, who has himself been profoundly influenced by the work of Morris and the English romantic tradition behind it.

The divergence between Thompson and Meier also reflects the pluralism of contemporary Marxism. Meier is committed to a positivistic and Leninist conception of Marxism: he believes that Marx discovered objective, scientific laws of history that remain valid. And he views the utopian projections of Morris as a plausible working out of the implications of those laws. Thompson, on the other hand, is convinced, like many recent Continental interpreters, that Marxism must be rescued

from its earlier formulation by means of the ethical and esthetic concerns found in Morris.

Meier's doctrinaire form of Marxism does not, in the end, yield a convincing interpretation of the socialism of Morris. It does not do justice to the moralistic and esthetic preoccupations that led Engels to dismiss Morris as an "emotional socialist." Indeed, Meier's reference to Morris in his subtitle as a "Marxist dreamer" brings out a basic irony in his study. It was the dream of utopia rather than contemporary economic and social forces that remained uppermost in the thought of Morris. Although his vision of communism coincided in a number of interesting ways with the scattered and more modest forecasts of Marx, Morris remained, as his purist attitude toward the political struggles of the workers indicated, firmly anchored in the older romantic tradition.

Still, Meier provides a richly detailed, searching, and scholarly examination of a utopian vision that continues to influence British socialist thought.

STANLEY PIERSON
University of Oregon

NORMAN MACKENZIE, editor. *The Letters of Sidney and Beatrice Webb*. Volume 1, *Apprenticeships, 1873-1892*; volume 2, *Partnership, 1892-1912*; volume 3, *Pilgrimage, 1912-1947*. Cambridge: Cambridge University Press, in cooperation with the London School of Economics and Political Science. 1978. Pp. xx, 453; xv, 405; xii, 482. \$125.00 the set.

Norman Mackenzie's selection of the letters of Sidney and Beatrice Webb will prove a valuable addition to libraries for students interested in British history from the 1880s to the Second World War. It will be a pity if general readers are deterred from acquiring them by the collective price of the three volumes. Justice could not have been done to their vast collection of letters in fewer than three volumes, which fall naturally into three sections: *Apprenticeships*, concerning their early lives and the early days of the Fabian Society; *Partnership*, dealing with the time of their most important administrative and political achievement in the phase of Fabian "permeation"; and *Pilgrimage*, showing the gradual disillusionment of the elderly sages with the "inevitability of gradualness" and their journey toward the rising star in the East.

Even so, these volumes make no pretension to completeness. Together they represent 967 letters out of "several thousand." Mackenzie's principles of selection are clear. Only letters written by the Webbs are included, not letters written to them. He has chosen to print the letters he has selected in full rather than to give extracts from a larger number. His preference has favored those that cast light on the character, intellectual development,

and attitudes of the Webbs, those that contain significant comments on other important personages, and those that add new information about the Webbs' special interests. The selection has also been made so that some continuity of themes might be sustained. Mackenzie develops these themes in neat historical overtures to each of the volumes and in a fine continuo throughout the headnotes to the letters, thereby providing the links with unseen correspondents, furnishing biographical particulars of the characters, and quoting extra material from Beatrice Webb's diary where it is needed. The reader will be filled with admiration of Mackenzie's skill in carrying out his editorial task.

How much that is new does this selection of letters tell us about the Webbs? Mackenzie claims that the letters "reveal much about the personalities of both Sidney and Beatrice that is markedly different from the received image of them" (p. xii). So far as Beatrice is concerned, this seems too large a claim. The letters appear to add little more than details to the fascinating picture of her that emerged from her lively and remarkable diary (still published only in part) and the two volumes of autobiography that she drew from it. But in Sidney's case, the claim is sound. The shadowy "Other One," who has been passed over as a reliable but philistine "lump of solid ability without any complications," emerges here still with all of his reliable ability but at a much more complex level, a man of great warmth and affection, far from being philistine or boring. The letters provide a sample of the depths of his interests that will no doubt be explored in the forthcoming biographical study by Royden Harrison and Ian Britain.

The letters also throw new light on the Webbs' intellectual partnership, a rare enough achievement in the marriages of intellectuals, and the devotion it involved. From the outset, Sidney was convinced that they could achieve more together than singly. He was right. The typical pattern in their society would have been for Sidney to have wed Beatrice's money and influence to promote his career as a radical politician, leaving her to pursue her literary interests as best she could. On those terms, they probably would not have married; but if they had, or if they had gone their separate ways, it is certain that their scholarly histories of British institutions would not have been written. It is on these works (of which, regrettably, we hear so little in the letters) rather than on their politics, that the Webbs' claim to their wreaths of laurel rests; and it is of these books that their fellow historians should also be generous enough to say: "Plundered by all, they still remain rich."

A. M. MCBRIAR
Monash University

ROGER MOORE. *The Emergence of the Labour Party, 1880-1924*. London: Hodder and Stoughton; distributed by Humanities Press, Atlantic Highlands, N.J. 1978. Pp. viii, 216. \$15.00.

Late nineteenth-century British "Labour" has been the topic of extensive research and writing since G. D. H. Cole's prolific contributions in the 1940s. Henry Pelling, in particular, has provided recent scholars with much to consult and absorb. Roger Moore follows the trend in asserting at the outset that he has "freely drawn" from the work of these two men, among others. Despite the absence of citations, it is obvious that he is well versed and up-to-date with both the primary and secondary material bearing on the evolution of the British Labour Party.

Moore provides a chronological account of the rise of the Labour Party between 1880 and 1924. In his conclusion he admits to the modest aim of "shed[ding] more light on two questions: why the Labour Party came into existence in the first place and how it came to establish itself in place of the Liberals as the alternative to the Conservatives." Neither of these aims would, at first glance, imply any new revelations or approaches. This is not, however, the case. The material presented in the body of the work shows that the author has not done justice to himself. Moore has, for example, effectively probed the "grass roots" of the British Labour Party. In the process he has cogently analyzed the local developments so vital to that rise. Much like Tom Forester's *The British Labour Party and the Working Class* (1976), Moore's discussion of the activities of the party at the local level is far more comprehensive than one would expect from a book designed for the general reader. In addition, he has accurately pointed out the rampant distrust of "leadership" within the labor movement and the resultant tendency of most authors to "play down the contribution of personalities in the party, stressing rather the interplay of social and economic factors." At the same time, he convincingly responds to this view by examining the rich assortment of characters who played notable roles in the story—especially Keir Hardie, Ramsay MacDonald, and Arthur Henderson.

One major shortcoming of the book is its lack of footnotes and the sparse bibliography. This failing is unfortunate; although based largely on secondary materials, the book provides a worthwhile synthesis that could otherwise be of great value to scholars of the British workers' movement. Aimed as it is at the general reader, it must be commended, nonetheless, for the well-chosen and attractive illustrations, the helpful chronologies of "Principal Events" at the end of each chapter and the short, summarylike conclusion. The author

also includes some useful appendixes, the best of which is the "Biographical Notes" on twenty-one of the "Labour" leaders.

Some may fault the weak handling of one topic or another, such as the pre-World War I syndicalist movement. Most should be pleased, however, at the insights offered into such topics as the study of local developments and the significance of Chartism as a factor in the emergence of the Labour Party. Overall, this work is a welcome addition to our knowledge of British working-class thinking in the late nineteenth and early twentieth centuries. It joins the Forester work and David Kynaston's *King Labour: The British Working Class 1850-1914* (1976) as worthwhile contributions to this field.

RAYMOND G. HEBERT
Thomas More College

JOSEPH L. WHITE. *The Limits of Trade Union Militancy: The Lancashire Textile Workers, 1910-1914*. (Contributions in Labor History, number 5.) Westport, Conn.: Greenwood Press, 1978. Pp. x, 258. \$16.95.

Joseph L. White's starting point is the famous assertion by Sir Henry Phelps Brown that industrial unrest in Britain in 1911-14 was largely confined to coalmining, railways, and unskilled workers generally, with textiles one of the industries little affected. In fact, cotton was near the top of the list of strike-prone industries, and White has compiled an admirable record of cotton strikes from official and unofficial sources.

He has also put together a valuable account of the structure of cotton employment at that time; of the main grievances of the workers; of trade union organization and policy; and of a number of interesting byways, such as employee shareholding in the mills and separate local unions of women weavers and Tory weavers. His overall, and convincing, conclusion is that unrest in cotton was due mainly to the conjunction of a number of specific pressing grievances, a prolonged boom, and responsive trade unions and that it owed nothing to syndicalism. The objectives and methods of the cotton operatives were untainted by revolutionary intentions.

Where the causes and consequences of the developments that he describes do not emerge readily from the evidence, White sets his fertile imagination to work. Ideas are always welcome, but his speculative powers may leave his readers behind when he writes that there were "40,114 married women of all ages who worked in cotton mills. Now if one assumes that all of them were married to the 74,247 men aged twenty or older who also worked in the mills . . ." (p. 28). The causes of the

biggest stoppage of them all, the lockout of weavers over the closed shop in 1911-12, have always puzzled labor history students, and they remain something of a mystery even after reading White's chapter on that topic, despite the suggestions that he offers.

He is not always fair to other authors. A substantial passage from Lord Asquith is quoted both on page 113 and on page 120, the second time correctly but the first time badly garbled. A criticism of Henry Pelling (p. 144) manifestly misses that author's point. White also says some plainly silly things. He finds it "significant that the weavers sought to obtain the closed shop not by legislation but" by industrial action (p. 148). Their action signified only that the weavers were not so far out of their minds as they would have had to be in 1911 to suppose that parliament would legislate a closed shop for them or any other trade union.

Such faults, however, irritating though they may be, by no means outweigh the merits of this study: a store of information, the definition and examination of a substantial issue in trade union history, a number of interesting ideas, and a readable presentation.

H. A. CLEGG
University of Warwick

BILL JONES. *The Russia Complex: The British Labour Party and the Soviet Union*. Manchester: Manchester University Press; distributed by Rowman and Littlefield, Totowa, N.J. 1977. Pp. ix, 229. \$21.50.

Whether or not the Soviet Union stands as "Labour's last Utopia" (p. 217)—Clause 4 remains firmly anchored in the party's constitution—Bill Jones has undertaken to assess the growing disillusionment with the Russian experience felt within Labour's ranks and, indeed, through the spectrum of the non-Communist left in Britain. His book traces the conflict between the "idealist" and "realist" approaches to the conduct of Labour's foreign policy, a struggle resolved in favor of the latter with the responsibility of government in the post-World War II years: the realist proclivities associated with Ernest Bevin's tenure at the Foreign Office have remained in the ascendancy since that time. Jones's theme is by no means novel—A. J. P. Taylor's *The Trouble Makers* has for twenty years stood as testimony to such conflicting tensions within the left in the debate over British foreign policy—but his particular focus upon Labour's response to the Soviet Union through the events of 1949 affords a revealing perspective of the party's dilemma. Nor does Jones ignore the intermediate positions occupied from 1917 to that time by other factions as well: none was immune from

what Jones, courtesy of R. H. S. Crossman, characterizes as the "Russia complex," namely the emotional attachment to the Soviet Union that had generated an acceptance of Russia as a socialist's utopia, a belief that until the early postwar years tended to mask the reality of totalitarian rule and the denial of democracy.

Although Crossman in 1948 contended that Labour no longer needed such a myth to compensate for its sense of frustration in failing to achieve social democracy in Britain—the 1945 election had given Labour its long-sought opportunity to build a new Jerusalem—Jones asserts instead the importance of the shattering of the utopian myth itself, through the actions of the Soviet Union in Eastern Europe, during and after the war. Thus what stood revealed to all save the most arid ideologues by 1949 was the distinction, in kind, not degree, between the ways of British social democracy and those of communism, whether of the Russian or CPGB variety. Jones's informative account traverses "the rise and decline of the enchantment" (p. vii) with the Soviet experience: his historical tour of the interwar years is unsurprising, although his comments are terse and to the point—the Webb's *Soviet Communism* is viewed as "a very silly book" (p. 19); the wartime twists and turns in response to the Soviet Union's initial perfidy and subsequent heroism Jones catalogues in greater detail; and the bulk of his excursion proceeds through the great divide of 1945–47 to the Soviet blockade of Berlin a year later, which produced a "massive" and unprecedented consensus within the party supporting Bevin's policy. Indeed, there was very little slippage of that consensus as Labour lined up behind the NATO alliance, an expression of power politics that only three years earlier would have been viewed as reactionary. In accepting the necessity for such an alliance policy, Jones affords little comfort to the revisionist camp of Cold War historiography, as he denies the assertion that Western leaders created a chimera of Russian expansion at a time when that war-ravaged country could not sustain armed conflict: "Soviet policy had all the *appearances*, however irrational, of expansionism" (p. 214), and realists had to respond to the perceived threat. Like the Labour Party, Jones accepts the validity of alliance politics by way of response; unlike the party, he recognizes that in so acting Labour had taken leave of the traditional tenets of a socialist foreign policy.

Jones's use of press and periodical literature is the most informative aspect of the book: it is well to have his record of the vagaries of left-wing commentary through these years, although his assertion that "as always in a crisis the *New Statesman* seemed to have lost its head" (p. 180) is a bit wide of the mark. The book is a straightforward histori-

cal account and thus will suit historians' tastes rather better than its nearest counterpart, Michael Gordon's typological *Conflict and Consensus in Labour's Foreign Policy, 1914–1965* (1969), but Jones's scope is obviously narrower. Nonetheless, the Russian case study demonstrates anew the inherent tensions in the creation and conduct of foreign policy in accord with abstract principles.

Regrettably, one must observe that Jones has not been well served by his publishers, who have permitted an unconscionable number of misspellings and typographical errors to mar the text. While *The Russia Complex* is neither strikingly original nor profound, it remains a useful and informative guide to Labour's exploration of the myth and reality of the Soviet Union.

JOHN F. NAYLOR
State University of New York,
College at Buffalo

PAT THANE, editor. *The Origins of British Social Policy*. Totowa, N.J.: Rowman and Littlefield. 1978. Pp. 209. \$17.75.

From 1850 to 1927, writes Norman McCord in the first essay in *The Origins of British Social Policy*, the rateable value of property in Newcastle rose sixteen times. This increase reflected an expansion of wealth that, despite ups and downs, was never to cease—an inexorable expansion that every decade made massive poverty less necessary and less acceptable, though, unhappily, not less a reality. The essays of Pat Thane on old age pensions, of J. H. Treble on the Glasgow unemployed, and John Macnicol on family allowances all attest to the depth of the continuing poverty. They also point to its causes: old age, unemployment, low wages, and large families. Treble's study of Glasgow offers a particularly close analysis of the various kinds of unemployment: large-scale unemployment during depressions, irksome seasonal layoffs, growing numbers of casual laborers as docks and building trades provide insufficient work, and the tragic redundancy of sail- and ropemakers in an age of steamships.

Joblessness was a tragic fate, but at least there was hope of a return to work, a hope not vouchsafed the aged. Their plight was pitiful. In 1906, writes Thane, 46.5 percent of those on poor relief were over sixty and half of them lived in workhouses. Dismal as these houses were, their diet was better than that of the children of the lowly paid. In York in 1936, writes Macnicol, half of those in primary poverty were under fourteen years of age.

From 1890 to 1940 ministers, MPs, bureaucrats, employers, trade unionists, philanthropists, journalists, and reformers searched for ways of lessening the massive poverty that expanding wealth had

not ended. For many the old remedies seemed best: workhouses, soup kitchens, exhortations for self-improvement, the discipline of the lazy, and the refusal of decent relief because it might corrupt. M. A. Crowther's essay on workhouses tells the melancholy story of how the Edwardians and their successors hung on to the Victorian belief that the mixed workhouse could deter the able-bodied from going on relief and at the same time reform orphans, care for the ill, and provide a decent place for the aged. In the 1929 Local Government Act the Conservatives finally abandoned that hope by abolishing the words "workhouse" and "pauper," but they kept many of the old edifices, which they still filled with orphans, the ill, and the aged. They gave up only on the unemployed, the masses of which in 1892-93, 1904-05, and 1921 destroyed all hope that a deterred able-bodied worker could find work. To aid the jobless Glasgow tried public works and soup kitchens, while the London parish of Poplar gave a generous and illegal outdoor relief. Glasgow's efforts failed, and a Tory government sent the Poplar guardians to jail.

Mass unemployment also destroyed new schemes. Unemployment and sickness insurance, begun on a limited scale in 1911 and expanded in 1921, collapsed in 1922 when nearly two million workers became jobless. The government thus turned to the dole but a dole purposely kept below the income of the lowest-paid worker, an income below what nutritionists said was needed for a healthy diet. For large families low wages or underemployment meant dire suffering, a suffering not really relieved until after 1945.

The story of British social policy from 1890 to 1940 is mixed and tragic, full of well-meant blunders and harsh callousness, unwitting ignorance and strong class biases, ratepayers' selfishness and bureaucratic pedantry, mounting wealth and unnecessary poverty. Though the eight essays in *British Social Policy* point to many reasons for that continuing poverty, there is one that is overriding: the stubbornly persistent belief that an adequate relief would corrupt the poor. A Glasgow circular, made out by employers in 1906, made it clear that personal failings accounted for only 10 percent of those who lost their job. World War I made it even clearer that the British workers were not malingerers. Yet even in the interwar years the governing classes persisted in the illusion that many of the poor were undeserving and lazy and that relief adequate to their needs would lure them from honest work. It was an erroneous belief but one that vitiated and paralyzed the thinking of the propertied classes until World War II again proved it false.

F. DAVID ROBERTS
Dartmouth College

PETER CALVOCORESSI. *The British Experience, 1945-75*. New York: Pantheon Books. 1978. Pp. 252. \$8.95.

This book seems designed to answer those oft-repeated questions about what happened to Britain, to its economy, and to its world role after the Second World War. It is not, however, designed for the serious student who wants to delve more deeply into the answers to these questions as it has neither index, bibliography, nor footnotes, omissions which reduce its effectiveness considerably.

The title, which suggests that attention will be paid to the social and cultural changes that have taken place in Britain since 1945, is somewhat misleading. Housing, poverty, and education are discussed but from a legislative and statistical viewpoint rather than that of their social impact. No mention is made, for example, of the effect of the upward mobility of working-class children generated by the educational reforms, or of the cultural impact of television (other than its use by the Open University). Social history is not its strong point. Fortunately, the author concentrates on the areas in which he clearly feels more comfortable, notably on the state of the economy and the effects of its weaknesses on various parts of the structure of Britain. His analysis of the trials, tribulations, and failures of the post-World War II economy is clear and intelligent, a valuable lesson for all those who think that the application of economy theory by the government will solve all problems overnight. He concludes "that the economic failures of our period were essentially industrial and that however the blame might be apportioned among different groups or classes, the main share rested on those with prime responsibility—the owners and their nominees or agents" (p. 191). Following his elegant discussion of the complexities of economic and legislative problems, such a conclusion seems a little harsh and oversimplified.

This is a useful analysis of the economic state of Britain since the Second World War and has good sections on foreign policy, but it lacks the wide-ranging view which would enable it to live up to its title.

JANET ROEBUCK
University of New Mexico

JOSEPH M. VICTOR. *Charles de Bovelles, 1479-1553: An Intellectual Biography*. (Travaux d'Humanisme et Renaissance, number 161.) Geneva: Librairie Droz. 1978. Pp. 191.

The freshest minds of the Renaissance—Petrarch, Machiavelli, Rabelais—ridiculed or rejected the philosophical and theological systems of the

Middle Ages. There continued, of course, to be schoolmen, but most of them were unable to (or wished to) do no more than defend the thought of the past as best as they could. An exception was the Picard canon and nobleman Charles de Bovelles, who understood that realist and nominalist thought was inadequate to the times. Basing his work on a refurbished Aristotle and an intellectualized mysticism drawn from Dionysius, Raymond Lull, and Nicholas of Cusa, Bovelles succeeded in creating an original philosophical and theological structure.

Joseph Victor has given us a definitive treatment of the life and thought of this early sixteenth-century French thinker. His biography provides an exact and judicious account of the currents that molded Bovelles's thought. In large part Bovelles followed the footsteps of his master, Lefèvre d'Étaples: humanized Aristotelianism, the mystical theology of Dionysius and Cusa, and the logic and rational theology of Lull. Master and pupil even suffered the same temptation—the austere life of the monk's cell. But the same path led the two men to a different end—in Lefèvre's case to the Holy Scriptures, in Bovelles's to metaphysics. The major part of Victor's work is a careful and systematic outline of Bovelles's metaphysics and theology. It is not the least of his achievements that he explains this complex structure in as clear and straightforward a way as possible.

The influence of Cusa on Bovelles is well known; Victor demonstrates that Lull's methodology was almost equally important. He shows how Bovelles used the *ars* of Lull to exhaust what could be predicated of the substance of God. In particular, he demonstrates the way Bovelles seized upon the relative predicates of Lull's logic to analyze the world of being and its relationship to God. Victor takes the trouble to place Bovelles not only against the background of late medieval mysticism but also the contemporary schools of scholasticism and Florentine Platonism. He shows how Bovelles's pious suspicion of the extramental existence of universals brought him close to the nominalists or, rather, ranged him against the realists. In thus placing Bovelles, Victor helps to explain why both he and Lefèvre came to reject Florentine Platonism.

Victor endeavors also to clarify the difficult question of Bovelles's attitude toward Lefèvre after the latter fell afoul the Sorbonne. It is obvious that Bovelles had no sympathy for the Reformation and no stomach for conflict, especially theological conflict. Still, Victor shows that the canon of Noyon remained steadfast toward his master if not toward his master's way of thinking. Indeed, whereas the latter moved toward a historical, evangelical theology, Bovelles rooted himself in a theology that was

pre-eminently ontological. In the final analysis Bovelles's thought—idealist, deductive, categorical—constituted an attempt to find a new justification for a view of the world that was essentially contemplative, immobile, and hierarchical. It seems to me that his success testifies to the fact that such a view continued to make sense in Renaissance France.

Bovelles's metaphysics and theology turn the rules of Aristotle's logic inside out. The system he built resembles nothing so much as the fantastic and dizzying constructions of early Renaissance French architecture, which seem to challenge the laws of gravity or the rules of proportion. Bovelles's thought represents a triumph of rationality, based as it was in the belief that there exists a fundamental harmony and affinity between man and the universe. Yet his arbitrary assumption of a correspondence between categories conceived by the human mind and the structure of the world is, in the final analysis, a wishful, indeed magical mode of thought. It is a way of thinking that links the canon of Noyon—philosopher, contemplative, aristocrat—with the world view of the most humble folk of his time.

HENRY HELLER

University of Manitoba

F. ELLEN WEAVER. *The Evolution of the Reform of Port-Royal: From the Rule of Cîteaux to Jansenism*. (Beauchesne Religions.) Paris: Beauchesne; distributed by Beauchesne-America, South Bend, Indiana. 1978. Pp. 176. \$13.00.

F. Ellen Weaver has undertaken a study of the reform of the convent of Port-Royal during the seventeenth century. She analyzes the *constitutions* and other regulatory documents pertaining to the convent as well as the spiritual development of one of its most prominent figures, Angélique de Saint-Jean Arnauld d'Andilly. Angélique reveals much about herself in her devotional writings and in her unpublished correspondence housed in the *Bibliothèque de la Société de Port-Royal* in Paris.

Since its founding in the thirteenth century, Port-Royal had been affiliated with the Cistercian Order. When Angélique Arnauld, the aunt of Angélique de Saint-Jean, launched her reform of the convent in 1609, her basic intention was to restore the Rule of Saint Benedict and to revive the austerity and penitential discipline associated with the Cistercian idea. Weaver emphasizes the point that the reform of Port-Royal was very much a part of the Catholic revival effected by the Counter Reformation, the full impact of which was not evident in France until the beginning of the seventeenth cen-

tury. Important institutional reforms at Port-Royal, such as the revival of the election of abbesses, were in the Cistercian tradition, but the spirituality of the nuns owed much to the influence of Cardinal Bérulle, one of the major figures of the French Counter Reformation. The adoration of the Eucharist—a significant Berullian theme—became an essential feature in the devotional exercises of the nuns. When the Abbé de Saint-Cyran, one of Bérulle's disciples, became the spiritual director of Port-Royal in 1635, his intense piety and his insistence upon a strict penitential discipline had a lasting impact upon the convent. His association with the convent, however, resulted in its fatal involvement in the Jansenist movement, which shaped its development during the latter half of the seventeenth century and led to its eventual destruction. In order "to enter more deeply into the inner history of the reform" of Port-Royal, Weaver analyzes the spirituality of Angélique de Saint-Jean, one of the nuns who bore the brunt of anti-Jansenist persecution during the 1660s. Angélique remained loyal to the Cistercian-Benedictine heritage and to the Berullian elements that had contributed to the reform of Port-Royal, but the Jansenist imprint was very much in evidence in her "great sense of personal mission to uphold and defend the truth of the Church" as she understood it. A significant effect of the nuns' association with Jansenism, according to Weaver, was the "gradual loss of the contemplative élan" that was vital to the success of the reform movement. Increased involvement in the Jansenist struggle for survival sapped the nuns' devotional energies. Had they retained a monastic indifference to the world, they might have avoided the disaster that overwhelmed them.

Weaver's study is an important contribution to the growing body of literature on Jansenism that has begun to appear in this country. It raises the crucial question of the relationship between that movement and Port-Royal. If Jansenism was essentially legalistic and disputatious, as Weaver and others (including Sainte-Beuve) assume, then Port-Royal's association with it indeed weakened its monastic zeal and damaged its spiritual integrity. If, on the other hand, religious commitment and the spiritual integrity of the individual were the essential features of Jansenism, Port-Royal must be regarded as an integral part of that movement. Assuming the latter to be true, as some historians do, one must conclude that Port-Royal was destroyed because its reforming ideals, along with those of the Jansenist movement as a whole, were not in tune with the social and political requirements of seventeenth-century France.

ALEXANDER SEDGWICK
University of Virginia

SHARON KETTERING, *Judicial Politics and Urban Revolt in Seventeenth-Century France: The Parlement of Aix, 1629-1659*. Princeton: Princeton University Press. 1978. Pp. xi, 370. \$22.50.

Three times in thirty years—in 1630, 1648-49, and 1659—the judges of the Parlement of Provence participated in open, violent revolts against royal authority. They harangued angry crowds in the streets of Aix and incited them to violence. They wore pistols over their red robes and marched against royal troops arrayed before their *palais de justice*. They arrested the provincial governor, the Comte d'Alais, and stood guard over him in prison. They converted peasants into "soldiers" and led them (ineptly) in a pitched battle. They were implicated in two murders. Like her predecessor, Lloyd Moote, Sharon Kettering has noticed the paradox of magistrates rebelling against the monarchical state which they were sworn to uphold. She has devoted this book to explaining the motives of these insurgent judges and to reconstructing their role in the rebellions.

By and large, the lines of explanation are political. Kettering stands in the tradition not only of Moote but also of Roland Mousnier and Georges Pagès; and her reduction of their general interpretations to the provincial level, though sometimes diffusely written, does yield interesting insights. The inveterate hostility between the Parlement and the intendants is portrayed in a particularly convincing way, while the tense relationship between the tribunal and the provincial governors is shown to have arisen much earlier in the seventeenth century than has previously been supposed and to have involved important issues of local political control. With respect to the role of the magistrates in the several upheavals in Aix, the author does much to open the closed doors of parliamentary sessions to historical scrutiny. She provides us with precise information about the magistrates' adverse reaction to the collapse of provincial privileges and about their personal ordeal in the 1640s when the crown undermined their investments in venal office by creating and selling new offices. In short, there was a wide variety of motives that brought the parlementaires into open revolt, ranging from anxiety over the question of increasing royal power in Provence to a self-interested concern for venal wealth. All of this leads to the essentially valid conclusion that the "success" of these parliamentary revolts was only temporary and that the crown "won" these political wars in the sense that at the death of Mazarin royal power in Provence had been considerably expanded and the magistrates themselves "transformed from a disorderly company of rebels into obedient, if sullen, servants of the king" (p. 330).

The most original feature of this book is its assertion that the revolt of the magistrates, political though its origins were, was to a lesser but still considerable degree motivated or carried along by parliamentary kinship groups: "Kinship and clientage ties were an important determinant of political behavior . . . [the magistrates] followed brothers and fathers-in-law into the streets of Aix . . ." (p. 250). Five kinship groups are mentioned (p. 220), and the participation of one or more of them in the various revolts is said to have affected the political role of the *parlementaires*. Kettering is the first scholar to attempt to work with this kinship theme in a sustained way, and she clearly regards it as an essential part of her book. One senses that her judgment is perfectly sound, but it is regrettable that she did not present her findings in a way that would be easier for her readers to follow. In particular, the use of tables and perhaps charts can be recommended and so can the adoption of some statistical controls to demonstrate the existence of these groups and to follow them through time. It is true that the kinship group headed by the president, Laurens de Coriolis, is reasonably well described and its part in the revolt of 1630 made clear; but it would have been advisable to have sorted out all the kinship groups more systematically throughout the book.

JOHN J. HURT
University of Delaware

DANIEL ROCHE. *Le siècle des Lumières en province: Académies et académiciens provinciaux, 1680-1789*. In two volumes. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques. Civilisations et Sociétés, number 62.) Paris: Mouton. 1978. Pp. 394; 520. 260 fr.

This rich and rewarding book is worthy of its provenance. Sponsored in part by the *Viième section* of the *École pratique des hautes études*, Daniel Roche's investigation of provincial academies in the eighteenth century is a major work in the sociology of culture. Its methodological base is quantitative, with nearly two hundred pages of tables, graphs, and maps to establish the statistical solidity of his research. But the nonstatistically-minded ought not to be put off. The statistics, along with the notes and the overwhelming list of sources, are confined to volume two. Volume one can stand alone as a lucid, insightful, and literate exposition of the social make-up, cultural role, and psychological resonance of the provincial Enlightenment's institutional structure. Roche has lived up to his assertion that, like Lefebvre, Labrousse, and Braudel before him, he believes in a social history in which "le chiffre ne déshumanise pas" (1: 187).

Using the extensive archives of thirty-two regional capitals, Roche has sought to grasp the essentials of academic culture in the provinces from its emergence as an echo of the *Académie française* to its decline on the eve of the Revolution. He has addressed himself to three principal areas of inquiry: definition of the cultural class constituting the academies, comprehension of the motives and vision animating the provincial republic of letters, and analysis of the sociocultural role of the academies as exposed by institutional behavior.

The work is organized into two parts: "Le Mouvement Académique" and "Société et Culture." In part one, Roche traces the evolution of the academic movement from 1680 to 1789, establishes the place of the academic institution in its urban milieu, and examines its relationship to power and to the Enlightenment, concluding with a portrayal of *l'homme académique* as the embodiment of the aspirations of a coherent cultural class. In part two, he analyzes the recruitment and social make-up of this class, lays out its relationship to such other manifestations of the provincial republic of letters as freemasonry and agricultural societies, and explores the intellectual content and direction of academic meetings and competitions.

Roche's central theme is that the academies provided equilibrium in a culture under stress. Dependent on official recognition at each step of its history, the academic movement furnished cultural justification for the monarchy and for the principle of a hierarchical social order. At the same time, however, in offering an increasingly open space for the recruitment of a new cultural elite, the academies countered hierarchy with a potentially egalitarian ideal. Although the history of the academies does not support the idea of a decline of the nobility, who retained a significant cultural role throughout the period under consideration, it does reveal the assimilation of a new "bourgeoisie of service," representing the clerical, medical, and legal professions, into a common universe of sociability and culture. Thus the academic movement both displayed and mediated the tension between established and emerging classes, between tradition and innovation, and between ideology and criticism. At the same time, the provincial academies aided in the formation of a national culture—transcending but not obliterating parochial cultures—which began as the creation of the monarchical state but which would prove even more congenial to the revolutionary state to come.

It is impossible to do justice in this space to the range and solidity of insights into the culture of the Old Regime contained in every chapter of this work. It must suffice to conclude by saying that Roche has added immeasurably to our under-

standing of the complex symbiosis between the Old Regime and the Enlightenment, which was at once its creation and its nemesis.

ISABEL F. KNIGHT
Pennsylvania State University

MICHEL C. PERONNET. *Les évêques de l'ancienne France*. In two volumes. Lille: Université de Lille III; distributed by Librairie Honoré Champion, Paris. 1977. Pp. 1,486. 120 fr.

Not since the work of the Abbé Sicard at the beginning of the century has anyone undertaken a major study exclusively devoted to the Old Regime French episcopacy. For this reason alone, the results of Michel C. Peronnet's research on the subject have been awaited with considerable interest. Unfortunately, Peronnet's completed *thèse d'état*, published as a reproduction of his typed manuscript, falls short of expectations.

In the initial sections of the book, the author traces the evolution of the family origins and the careers of the bishops between 1516 and 1789 by means of a massive study of genealogy and ecclesiastical patronage. Particular stress is placed on the increasing standardization of the training of future bishops and on the growing domination of the episcopacy after the mid-seventeenth century by an "oligarchy" of "cousins" within the *noblesse de race*. On the question of changing social origins, the author goes beyond the studies of Norman Ravitch, Marilyn Edelstein, and J. Michael Hayden, first in his more detailed chronology, and second in his linking of the older aristocracy's reconquest of the episcopacy to the introduction of a requisite pre-episcopal training stage as vicar-general. Since the vicars-general were named by the prelates themselves, the latter won co-optive control over the pool of candidates from which the king might choose new bishops. Though we would differ over certain aspects of his analysis, the overall interpretation seems persuasive, and the data presented on family milieux and patronage will be of considerable value for the social historian.

In contrast to the trisecular scope of the earlier sections of the book, the chapters dealing with the bishops' political and ecclesiastical activities focus only on the last half-century of the Old Regime. The well-worn thesis of an aristocratic reaction is mobilized as the framework for a lengthy chronicle of the General Assemblies of the Clergy after 1740. For Peronnet, there was, at this time, a broad resurgence in the political opposition of the episcopacy toward the monarchy, a resurgence that must be closely related to the growing independence and homogeneity of episcopal recruitment. Although such a hypothesis cannot be altogether

discounted, the evidence presented here may well impress the reader with the essentially *defensive* nature of the bishops' actions: against the fiscal initiatives of the monarchy, against the Parlements' attacks on the tithes, against the "revolt of the curés," against the onslaught of Enlightenment anticlericalism. Yet the greatest disappointment is in the treatment given to the ecclesiastical activities of the episcopacy. Such major questions as the bishops' pastoral visits, their relations with their curés, and their involvement in the Jansenist controversy are passed over in a few paragraphs. We learn nothing at all of the libraries they assembled and almost nothing of their general religious and secular culture. On the critical problem of episcopal residence, Peronnet is confusing and contradictory and contributes scant new evidence. Perhaps part of the difficulty stems from the restricted nature of his sources, limited with rare exceptions to such printed documents as the collections of the Clergy of France and the *Nouvelles ecclésiastiques*. Even the manuscript archives of the *agence-général* and the massive church archives preserved for several dioceses of France have been largely or totally unexplored.

The text itself (1,500 pages) is repetitive and badly in need of editing. Since there is no index, the immense erudition in genealogy will be accessible with difficulty to the local scholar and biographer. Although one cannot but welcome the author's stated goal of breaking through the stereotypes and achieving a more positive assessment of the eighteenth-century bishops, much of his examination of their culture and pastoral activities is scarcely less impressionistic than the very work by the Abbé Sicard, which he had hoped to surpass.

TIMOTHY TACKETT
Marquette University

FRANÇOISE MOSSER. *Les intendants des finances au XVIII^e siècle: Les Lefèvre d'Ormesson et le "département des impositions" (1715-1777)*. (Mémoires et Documents Publiés par la Société de l'École des Chartes, number 23.) Geneva: Librairie Droz. 1978. Pp. xxvii, 327.

Despite the crucial importance of finances to the coming of the French Revolution, the *intendants des finances* are among the most poorly known of all the officials of the ancien régime. Françoise Mosser corrects this oversight in her revision of a *thèse* originally submitted to the Ecole des Chartes in 1967.

In keeping with the practice of current French administrative history, Mosser asks two basic

questions: Who were the intendants and what did they do? She first examines the personnel of the office—the twenty-six intendants during the eighteenth century—using detailed genealogical information to explain their position within the governing elite of French society. Next, she attempts to give as precise a description as possible of how the office functioned, using as an example the department of “impositions” held for over sixty years by the Ormesson family: grandfather Henri I, father Marie, and son Henri IV. This detailed analysis rests primarily upon the Ormesson family papers now on microfilm in the Archives Nationales.

Mosser’s minute description of the intendants’ activities will impress all serious students of French administration, but the general reader will be most interested by her basic conclusions. The intendants of finance, technically the subordinates of the *contrôleur général*, held an immense amount of power. Mosser agrees with her mentor, Michel Antoine, that the *contrôleur général* and his assistants took away from the various governmental councils basic control over fiscal matters. By the end of the eighteenth century, these councils, even the *conseil royal des finances*, met irregularly or not at all. They served only as rubber stamps to approve decisions already reached by the king with the *contrôleur général* and with the intendants of finance.

It was as heads of departments within the finance ministry that the intendants wielded real power. While *contrôleurs généraux* rose and fell (there were nineteen between 1715 and 1775), the intendants owned their offices and often held them for years. The result was a stable bureaucracy possessing considerable fiscal expertise. Consequently, the intendants became quasi-independent “mini-ministers” who ran the business of their own departments without interference and met frequently with other intendants to reach common agreement over policy. Even when the most important decisions were referred to the *contrôleur général*, the minister usually followed their advice. The result was a group of *techniciens*, specialists, and indispensable collaborators who emerged as powerful men behind the scenes. This very power led Necker to abolish the office in 1777—too quickly in Mosser’s opinion, for he destroyed a deep-rooted system that had assured a measure of confidence in the fiscal administration without replacing it with a viable alternative.

In brief, this work meets the high standards one expects from French scholarship—thorough research complete with charts and detailed biographical notices. It will remain indispensable for anyone wishing to understand the fiscal machinery of the ancien régime.

DOUGLAS CLARK BAXTER
Ohio University

JOSEPH N. MOODY. *French Education Since Napoleon*. Syracuse, N.Y.: Syracuse University Press. 1978. Pp. ix, 252. \$15.00.

A comprehensive history of French education in modern times has not previously been available in English. Few people are better qualified to write such a book than Joseph N. Moody, and he has succeeded in producing a concise but authoritative guide. He brings to the task his specialized knowledge of French religious history and he uses some archive sources for the Second Empire period, but his general aim in this book is to synthesize modern scholarly work. Moody overlooks very little indeed, gets all the details right (although the number of misprints of French words and names is rather high), and is as familiar with contemporary educational debates as with those of the past. Although he brings the story down to 1977, Moody avoids the tendency, to which educational historians seem especially prone, to judge the past in the light of the present. He is a historian of France rather than of education alone, and he explores with acuteness and depth of understanding the relation of education to social change, to political conflict, and to intellectual and religious developments.

The very fact that education touches on so many facets of a nation’s life poses problems of method for the historian. Since French education has always been highly centralized and has never been far from the heart of politics, its historians have always concentrated on public debate, Church-state relations, and changes in government policy rather than on what was happening in the schools themselves. Moody, too, writes within this tradition. He adopts a fairly strict chronological approach, with five chapters marked off by changes in the political regime, and he does not fully solve the problem of integrating descriptive material and discussion of general issues. Sometimes the chapters jump rather confusingly from one theme to another, and in places an unevenness of treatment reflects the current state of research. It seems a pity that Moody did not find room for more expansive and analytical treatment of such matters as the development of Catholic schools, the classical tradition, the growth of literacy, or the values and unconscious assumptions so powerfully transmitted by primary schools and their teachers.

If the book has a leading theme, it is the “compartmentalized” nature of the system’s historical development, with a basic education for the masses growing alongside an elitist secondary system where the classics served as a social barrier and as a means of regulating who crossed it. It is fair to say that Moody gives more space to the bourgeois sector of education, which he finds a

more congenial field for discussion. He argues that no fundamental change in the elitist nature of French education took place until after 1945. This theme forms the main subject of his final chapter, which is inevitably more speculative than the others. Some readers may find his judgment of the Fifth Republic overindulgent, and he is far from starry-eyed about the events of 1968.

R. D. ANDERSON
University of Edinburgh

STUART L. CAMPBELL. *The Second Empire Revisited: A Study in French Historiography*. New Brunswick, N.J.: Rutgers University Press. 1978. Pp. xv, 231. \$14.95.

Specialists in modern French historiography have often called attention to the powerful influence that politics has exerted on the writing of history in France. Several studies of the relationship between the historian's politics and his work have focused on the changing interpretations of a particular period of the French past in the writings of successive generations of historians. Paul Farmer's *France Reviews Its Revolutionary Origins* highlighted the tendency of historians of the French Revolution to view that phenomenon from the narrow perspective of their particular political involvements. Pieter Geyl's *Napoleon, For and Against* reached a similar conclusion about the historiography of the First Empire. Stuart L. Campbell's book applies this method of analysis to the historians of the Second Empire. The author selects a handful of representative French historians of that epoch, supplies brief biographical sketches to situate each writer in his political context, and proceeds to expose the ideological influences on these interpreters of the legacy of Napoleon III.

Those historians writing in the decade after the collapse of the Empire understandably exhibited the most partisan attitudes toward the regime under which they had acquired their political consciousness. The liberal republican Taxile Delord portrayed Louis Napoleon as the despicable assassin of the revolutionary movement and his political system as an unpopular autocracy propped up by military force and bureaucratic regulations. The liberal monarchist Pierre de la Gorce, traumatized by the Commune and troubled by the Third Republic's anticlericalism, hailed the Liberal Empire of the 1860s for having restored the Orleanist synthesis of liberty and authority disrupted by the revolution of 1848.

This preoccupation with the political institutions of the Second Empire continued beyond the turn of the century, as scholarly apologists for the Third Republic strove to legitimize France's democratic system through favorable comparisons to

its predecessor. The republican Charles Seignobos updated Delord's denunciation of the imperial autocracy as the enemy of the very political liberties that had been restored after its demise. The socialist Albert Thomas, whose political career had been devoted to forging an alliance between the working class and the republican state, dismissed the Napoleonic social program as a cynical effort at co-optation, designed to divert the nascent proletariat from an alliance with the republican opposition.

It was only after the First World War, Campbell conclusively demonstrates, that French scholars began to appreciate the positive aspects of the Second Empire's policies of social reform and economic modernization. Faced with the unraveling of the "stalemate society" of the Third Republic, historians such as Marcel Blanchard and Georges Duveau rediscovered in the Saint-Simonian technocracy of the 1850s a model for a possible alternative to the political polarization, economic stagnation, and social unrest of the interwar period. The Bonapartist principles of executive supremacy, corporatism, and economic planning acquired a new respectability among the generation of historians that had repudiated the Third Republic's ideology of legislative sovereignty and economic liberalism. The result has been an increasingly favorable evaluation of the Second Empire that continues under the technocratic, presidential system of the Fifth Republic.

WILLIAM R. KEYLOR
Boston University

ZEEV STERNHELL. *La droite révolutionnaire, 1885-1914: Les origines françaises du fascisme*. (L'univers historique.) Paris.: Editions du Seuil. 1978. Pp. 441.

This is the second installment in Zeev Sternhell's venture to write the history of what he calls variously the "revolutionary right," "radical right," or "national socialism" in France. It all adds up to fascism, and the author is fond of repeating that the ideas which European fascists claimed to invent in the 1930s were already a half century old. Sternhell's project began with his *Maurice Barrès et le nationalisme français* (1972), a study of the alienated patriot whose ideas and development appear to provide the conceptual framework for the present volume as well. Forthcoming is a book on the fascist movements of the interwar period. The work reviewed here attempts to provide fascism with an intellectual history as a "right-wing tradition fundamentally opposed to the liberal and democratic consensus," to place its origins in the heated coupling of nationalism and socialism during the Boulanger affair, and to trace its evolution through three generations between 1886 and 1925.

Sternhell sees a pattern of failure in the successive assaults of these generations upon liberal democracy in France. Each generation rises up more radical than the last, each exhausts itself in struggle against a "stalemate society" structured to contain its enemies, and each then backslides toward the political center, "losing itself in conservatism." In brief, the path traveled by Barrès from revolt to conservatism has here been widened into the road of the whole radical right.

For Sternhell, France was the original laboratory of the revolutionary right. There it expressed itself in a richness and variety of ideas that no other nation could equal, and there, as a result, modern scholars can find the best materials to construct a model of European fascism.

Sternhell has absorbed a prodigious sum of information from police records, newspapers, and political literature, and his work is the most remarkable enterprise presently underway to comprehend the world of radical nationalism in France. Primarily it is intellectual history, and the ideas described form the familiar fascism of instinct and unreason. In the final pages, however, this creed is declared to possess another side as well, "a fascism pragmatic, technological, and managerial," but this does not complete the story so much as create the impression that part of it has been left out in the preceding chapters. In contrast, Sternhell labors throughout to map the left-wing tributaries of the revolutionary right, rediscovering in the process an influence heretofore happily forgotten by most historians: Pierre Biétry and Jaune syndicalism. Regrettably it must be said that the predominant "fascist" personalities from the left during the 1930s, Jacques Doriot and Marcel Déat, appear to have little connection to the traditions that Sternhell describes. Indeed it can be objected that his entire venture to establish direct lines of ideological transmission between so-called prefascists and fascists leaves too many loose wires and faulty connections. Some years ago Edward Tannenbaum criticized Sternhell in this journal for assuming without direct evidence, *post hoc, ergo propter hoc*, that a causal link existed between Barrès's ideas and later fascist ideology. The criticism did not take, but the point is worth repeating.

GILBERT ALLARDYCE
University of New Brunswick

WALTER A. MCDUGALL. *France's Rhineland Diplomacy, 1914-1924: The Last Bid for a Balance of Power in Europe*. Princeton: Princeton University Press. 1978. Pp. xiii, 420. \$25.00.

Walter A. McDougall's vivid portrayal of France's Rhineland diplomacy is presented in authoritative

style, but those who have read the evidence will find his picture divorced from reality. Not only is his interpretation at variance with the archival data, but a multitude of mistakes in names, titles, and dates, and many substantive errors in the bibliography, undermine his credibility. Even the map is inaccurate.

One problem is inadequate research. While McDougall lists many archives, he writes largely from Millerand's papers, French Foreign Ministry Series Z (Europe) Rhineland files, and a few volumes of Series A (Peace). The wartime records; Z files on Belgium, Britain, Germany, and the Ruhr; the Commercial Relations and International Series; and vital private collections are used sketchily or not at all. For periods when few relevant Quai d'Orsay files survive (1914-18 and 1920), McDougall rarely compensates from other archives; instead he depends largely on secondary sources of uneven reliability. Use of British, German, and Belgian documents is cursory. Reading the main Belgian Foreign Ministry file on Rhenish separatism (10.440-3) might have corrected McDougall's notion that Jaspar favored it and might have raised his low accuracy level on all matters Belgian. More reference to conference minutes could have eliminated confusion of the early 1921 Paris and London conferences, rectified claims that the Schedule of Payments was completed before the April-May 1921 conference began, and prevented description of the Cannes Conference without mention of reparations. Additional time in London might have corrected McDougall's view that Lloyd George preferred Poincaré to Briand, reduced the level of error regarding reparations, and unscrambled his account of negotiations leading to the Dawes Committee.

Unfortunately, McDougall also handles evidence carelessly. He misrepresents the December 1921 Anglo-French talks and the September 1923 Baldwin-Poincaré encounter. Curzon's December 1921 memo on a possible French alliance becomes "his reaction" to Poincaré's January 1922 draft treaty. McDougall seems to think all evidence is of equal value and all individuals of equal importance. He accepts a March 1920 report that the Vatican would convert the Munich "nunciature" into a Berlin "embassy" (to embrace the SPD, not "Friedrich" Kapp). In 1923 Foster Dulles becomes an official American spokesman and the self-appointed Arnold Rechberg a serious emissary. Questions become proposals; recommendations, even if ignored, constitute policy; raw intelligence reports become established facts; routine staff papers become General Degoutte's opinions. Worse, evidence is distorted or suppressed. In reporting that Jaspar favored Rhenish de-Prussianization, McDougall claims that he refused to pronounce on the proseparatist Nothomb's plans and deduces

Belgian government intrigues, suppressing from the same telegram Jaspar's declaration that Nothomb's policies were illusory and dangerous to Belgium and Nothomb's agent's complaint of government inaction. An August 1923 Poincaré telegram approving refusal of aid to Dorten becomes by implication endorsement of active support.

Much data and many quotations are undocumented. Some material could easily be footnoted from secondary sources, some is confused, and some seems the product of a sprightly imagination. The results are startling: Clemenceau appointed new bishops in Alsace-Lorraine in 1919; the Little Entente was aimed against Hungary and France; European war debts were *not* owed to the American government; and "Mussolini led his 60,000 Blackshirts into Rome and prevailed upon Victor Emmanuel to appoint him" (p. 233).

As the foundation is unsound, so is the superstructure. McDougall incorrectly asserts that the French Chamber rejected the Wiesbaden Accords and builds much on this instead of seeking the real reasons for their failure. He contradicts and disproves himself repeatedly. Dorten was both "suited to lead" the Rhenish movement and a nonentity. Briand seems simultaneously for and against a Ruhr occupation. McDougall has reflected little on his data and misconstrues or ignores its implications. His views change from chapter to chapter (as do his facts), and conclusions about policies shift accordingly. While some nuggets exist among the dross, only an expert can distinguish them.

McDougall's analysis of the Rhineland problem is murky. Rhenish attitudes remain obscure; "the Rhenish policy" seems to cover support for separatism, autonomy, federalism, and integral treaty execution. McDougall cannot decide whether Poincaré and Degoutte favored separatism nor whether France wanted treaty revision or a static policy of treaty execution. While ultimately indicating a diversity of views, he confusedly concludes, "The Rhenish policies of France were forged in 1923 into the cutting edge of French revisionism, the bid for a static European polity . . ." (p. 366).

It is a great pity. A solid study of the Rhenish question is needed but, alas, this book is not it. With its seductive prose, apparent scholarly apparatus, and welter of inaccurate detail, it will only mislead the unwary.

SALLY MARKS
Rhode Island College

ANTOINE PROST. *Les anciens combattants et la société française, 1914-1939*. Volume 1, *Histoire*; volume 2, *Sociologie*; volume 3, *Mentalités et idéologies*. Paris:

Presses de la Fondation Nationale des Sciences Politiques. 1977. Pp. 237; 261; 268. 330 fr the set.

These three volumes are a masterful combination of social and intellectual history. Antoine Prost's command of quantitative data (the statistical tables are as imaginative as they are elaborate) is matched by a linguist's alertness to verbal nuances and mystifications (the textual analyses of veterans' speeches are as unsentimental as they are subtle) and a psychologist's feel for emotional intricacies. The first volume deals with the politics of the veterans' movements, the second with their social bases, and the third with the veterans' "mentality."

Prost contradicts a number of myths about French war veterans—myths perpetuated by veterans themselves. The fraternity of the trenches was often outweighed by egoism and indifference, real intimacy was inhibited by the emotional hardness demanded by war, and friendships made during the war seldom lasted after the armistice. In the trenches soldiers were morally no better nor worse than they had been in civilian life: it was "an average humanity" who fought the war, and the war did not elevate their souls. Although they knew this as soldiers, they conveniently forgot it as veterans, indulging in a sense of spiritual superiority toward those who had not fought and believing in a "unity of the trenches" that had not existed because otherwise all the suffering they had endured would have seemed in vain. Their sense of superiority was not romantic, however, for they remembered their fear more than their courage; they had been *pauvres bougres* cowering in the mud, not dashing heroes wrapped in glory. Nor were they flag-waving nationalists after the war, being much less patriotic than their American Legion counterparts: *la patrie* owed them far more than they owed *la patrie*. The war made them pacifists and the military antimilitarists. They had a thoroughly civilian dislike for arbitrary and mechanical discipline (they refused to march in step in veterans' parades) and a thoroughly egalitarian hatred of arrogant and ambitious officers (all postwar saluting was inimicable to them). These characteristics separated the veterans' mystique from the mystique of the French fascist leagues of the postwar period.

In the diverse veterans' associations that emerged after the war there was often a sharp contrast between rhetoric and reality. Theoretically apolitical, "above" religious divisions, and devoted to transmitting the unity of the trenches to civilian life, they were fundamentally political and torn by serious ideological divisions of their own—incompatibilities that they tried to smooth over with purely verbal syntheses. There was not one veterans' movement but several, ranging from the

right-wing UNC, to the centrist UF, to the left-wing FOP, FNBPC, and ARAC. Prost uncovers the social bases of these divisions. The UNC was dominated by upper bourgeois and by middle and lower bourgeois of the private sector who shared the same social ideals. The UF had a much higher percentage of members from the public sector, middle and lower bourgeois who sought upward social mobility through teaching and government service. Both the UNC and UF, the two largest veterans' associations, had a large peasant base, yet leadership positions were monopolized by the urban middle classes. Ideological discord between the UNC and UF was rooted in social antagonisms between the private and public sectors, established notables and the recently arrived, and Catholics and secularists. The bottom of the social pyramid was represented by the Communist ARAC, but even it was more lower bourgeois than proletarian. Leaders led only insofar as they did not stray too far from what their followers wanted. When Paris UNC leaders flirted with fascism in 1934, they were soon reprimanded.

The ostensibly apolitical UNC was political from birth. Supported by church and army leaders, subsidized by Clemenceau and big business circles, and its ceremonies regularly blessed by priests, it opposed the railway strike of 1920 (encouraging UNC members to serve as blacklegs), protested Cartel des Gauches anticlericalism, was cool to the Popular Front, and defended "all the traditional principles of the Catholic Right." The UF began on the left (its first Secretary-General was a member of the CGT) but evolved to the left-center. It was more sympathetic to the railway strikers of 1920 and to the sitdown strikes of 1936, was a friend of the Popular Front, and was firmly opposed to any compromise with fascism. Like the UNC, however, the UF was also committed to order and legality and opposed to revolution. Both the UNC and UF were forces for moderation and reconciliation during the depression, middle-class movements that wanted to calm social conflict between management and labor. According to Prost, they also undermined French fascism in the 1930s by depriving it of supporters with their stand against extremism (although, oddly, Prost states elsewhere that veterans had little influence on French politics during the period). During the "fascist riots" of February 6, 1934, Georges Lebecq of the Paris UNC mobilized 20,000 veterans to demonstrate with the Croix de Feu and other leagues against the Daladier government, but the bulk of French war veterans, including the UF and the provincial sections of the UNC, refused to participate. Prost emphasizes that most veterans were antifascist in their distaste for dictators, militarism, and civil war and in their attachment to

republican liberties, social harmony, and petty bourgeois epicureanism. When, after February 6, veterans' leaders called for a "Reform of the State," they anticipated the Fourth and Fifth Republics more than they echoed the Third Reich, and, even here, they were closer to Mendès France than to De Gaulle.

Prost's conclusion that the veterans' movements were antifascist, although largely true, is stated too categorically to accommodate parts of his own analysis. For he also shows how veterans repeatedly denounced parliamentarianism, pictured politicians as corrupt and themselves as a moral elite (devaluing electoral democracy in the process), praised the "brutal" virility of the soldier and damned the soft hedonism of the civilian (an asceticism that clashed with "epicureanism"), and at a congress of the UNC in 1933 applauded a "moderate" delegate who praised Mussolini and Hitler for saving their countries from "anarchy." Prost concedes there might have been a "germ of fascism" in some of this. He also notes that veterans' leaders sometimes exploited the "inertia" of their followers for extremist ends and that by denigrating the politicians of the Third Republic they "prepared the way for Marshal Pétain." Prost's acceptance of the stereotype of French fascism as being more socially radical than it really was (it too appealed for "reconciliation" on middle-class terms) is unfortunate. But on the whole this is history at its best—well documented, lucidly written, and full of insights.

ROBERT SOUCY
Oberlin College

COLIN DYER. *Population and Society in Twentieth Century France*. New York: Holmes and Meier. 1978. Pp. 247. Cloth \$23.50, paper \$12.00.

Colin Dyer has undertaken the ambitious project of writing a general history of the French population since 1911. Beginning with the state of the population on the eve of the First World War, he describes trends in the movement of the population and their relationship to the major events of the twentieth century. The slow growth of the years before and after the Great War, the losses through deaths and lost births in that conflict, the failure of the postwar population to recoup these deficits, the aging of the French population, the post-World War II movements of baby boom, and, after 1962, decreased fertility are each considered in turn. His account traces the impact of these demographic movements on the military and social institutions of France: the increasingly desperate position of France in a conflict with Germany is the principal background for the early parts of

the book, and his chapter on the baby boom after World War II examines the effect of this "bubble" on educational and housing facilities during the 1960s. A major contribution of the book is to make accessible a mountain of statistical data on the French population in this century. Those seeking a summary statistic will probably find it here.

Unfortunately, the mass of statistics remains largely undigested. The book is written from a strong but unexamined pronatalist viewpoint, and in his alarm at the low growth rate of the French in the years before and after World War I Dyer approaches the hysteria of French demographers and politicians in those years. He emphasizes antinatalist attitudes as the explanation for the slow growth of the population yet does not appreciate the ability of the French family to adjust its growth to short-term economic conditions. Neither does he test the employment and income hypotheses that have explained so much of the fluctuation in births in the United States in this century. The baby boom, for example, is attributed largely to family assistance programs, without consideration of the impact of the postwar resurgence of the French economy. Only rarely does he attempt to locate either demographic attitudes or behavior in the different classes of French society; whether the working class experienced fertility and mortality patterns different from those of the peasantry or the bourgeoisie is not considered. Finally, there is an annoying misuse of technical terms. Fertility, the actual births in a population, is persistently called fecundity (the physiological capability of women to bear children). He refers (p. 27) to the French population in 1911 as stable (a population that has had the same mortality and fertility schedules for a long period of time) instead of stationary (one with as many deaths as births each year).

Dyer has provided a compendium of statistical data on the French population since the Great War. His book, however, serves as a reminder that this period remains a fertile area for research in population history.

JAMES R. LEHNING
University of Utah

JANE MARCEAU. *Class and Status in France: Economic Change and Social Immobility, 1945-1975*. Oxford: Clarendon Press. 1977. Pp. 217. \$13.95.

France, like other European countries, has experienced its economic "miracle" in the postwar era. The society has scarcely lived up to the pessimistic forecasts of the early 1950s by Landes, Luethy, and the contributors to the volume that Edward Mead Earle edited in 1951 entitled *Modern France*. Today,

France is one of the leading industrial nations in the world and, as a consequence, one of the richest.

Jane Marceau's *Class and Status in France* takes as given France's spectacular economic growth over the past thirty years. Her concern is with the effects of the increasing wealth of the society on the social structure. Her central thesis is that, "although after 1945 economic changes, allied to certain social and political ones, greatly improved the living standards of the mass of French people, they nevertheless did little to modify the shape of the social structure" (p. 1). Advantages and disadvantages were both cumulative, so that the wealthy had a better chance of obtaining a good education, of enjoying higher status, of living longer, of enjoying power, while the poor could aspire to none of these.

Marceau argues that the inequalities that inhere in French society are not fortuitous. They have a structural basis deriving from the nature of the production process. She assembles considerable data on the distribution of wealth, mortality rates, housing, professional (im)mobility, access to higher education, and the social origins of different sectoral elites. All of these data, according to Marceau, point to persistent inequalities in the French social structure that have remained unaffected by the increasing wealth of the society. Marceau does not believe that the rigidity of the social structure is unique to France. It is to be found in other societies possessing similar economic relationships.

Class and Status in France is a provocative work and not everyone will agree with either the framework adopted by the author or the conclusions reached. This is not least because the data presented are open to different interpretations. Marceau must be commended, nonetheless, for making sense out of a mass of data scattered in official documents and in the works of many French sociologists. Her elegantly presented synthesis will be extremely valuable to all those working on contemporary French society.

EZRA N. SULEIMAN
University of California,
Los Angeles

TONY SMITH. *The French Stake in Algeria, 1945-1962*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. 199. \$15.00.

I guess we must excuse historians for being historians and allow them to make a book from the reevaluation of old material or the grouping of it around a new theme. Many years ago Richard Brace and I wrote a book on French Algeria (*Or-*

deal in Algeria) and one of our colleagues facetiously suggested another title for it: *Algeria: French or Free?* Though we did not use the title, it admirably sums up the crux of the matter discussed in Tony Smith's new book *The French Stake in Algeria*. Smith maintains that it took an eight-year war for Algeria to gain its independence from France because there was a consensus among Frenchmen and especially their politicians and governors that Algeria had to be a part of France for that country to remain a world power, the legendary France of "La Gloire." France in the 1940s was deep in the humiliation of the aftermath of World War II; later came the loss of Indochina, an inconceivable thing. And wasn't there even 1870 to brood upon? Absolutely no more losses could be tolerated.

Now all this is perfectly true, if hardly a new thesis. More interesting is the material Smith has mustered to explain why the so-called liberal elements in France, the French Communist Party, and the Socialists (SFIO) were unable to move France from this consensus to a realization that the events and the time pointed toward the eventual victory of nationalist forces in Algeria. This he elucidates by pointing out the schizoid nature of French political life. For centuries France has teetered back and forth between its two traditions—a liberal and revolutionary one, and another historically cherished from the times of the divine right of kings and reincarnated in later "men on horseback" such as Napoleon and Charles de Gaulle. The latter has held that France could be generous in sovereignty, liberal under a strong central and patriarchal government. These two French traditions are obviously in conflict, and Smith sees in this conflict the weakness that contributed to France's loss of Algeria. Given the intensity of French repression and the bitterness of the struggle, this point of view is, I believe, a bit shaky. So is another of the points Smith examines: France lost Algeria for reasons of salutary neglect rather than for reasons of economic chauvinism, land stealth, and a complete incapacity to act in concert with the Muslims to help them enter the world of modern economics. It would not seem that the wholesale banning of Algerian political parties, the imprisonment of the leaders (even those opting for federation or assimilation), rigged elections, the government's response to settler lobbying, violent repression of nationalist demonstrations as at Sétif, organized torture and terror, and at last the commitment of over 600,000 soldiers to Algeria represented a policy of salutary neglect or one of weakness.

Smith discounts the idea that it was the weakness of the Fourth Republic—whose governments saw even the smallest concessions to Algeria voted down or themselves voted out of office because of

them—that kept the struggle going. He argues rather that France, like Britain under Winston Churchill, did not choose to divest itself of empire, and particularly not to the United States, Great Britain, and the new powers of Egypt and the Soviet Union. Those men in politics who actually fought for a liberalization (within safe confines) of Algerian policy did not last long in office. Blum, Mendès France, and even the de Gaulle of the Resistance period were unable to move with much certainty in that direction.

Smith maintains, contrary to statements we have heard from the GPRA and the FLN, that the French Communist Party was favorably disposed toward Algerian nationalism. It was, but only to a point. The Communists felt they had more to lose than to gain by deserting the middle of the road parties on this issue. And the Socialists activated the harshest measures of all against the Algerians.

Smith wonders if de Gaulle could have acted a year or so earlier in according independence to Algeria, thus avoiding the army putsch and the worst atrocities of the OAS. But he had a long way to go, step by step, to bring himself and the country to reverse their former stubbornly held convictions (against a free Algeria) and take a new path on which lay not only common sense but also reason and French honor.

JOAN BRACE
Lake Orion, Michigan

DOMINICK LACAPRA. *A Preface to Sartre*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. 250. \$12.50.

Dominick LaCapra's book offers an interpretation of Sartre based on a selected use of the critical strategy of Jacques Derrida. The "deconstruction" of Sartre is valuable both as a commentary on existential Marxism and as an example of Derridean criticism.

LaCapra restricts himself to Sartre's philosophical works, his existential biographies and autobiography, and to *Nausea*, leaving aside the political essays, the plays and short stories, *Roads to Freedom*, interviews, and literary criticism. LaCapra is best with *Nausea*, *Being and Nothingness*, and *L'Idiot de la famille*. Here he presents convincing analyses by employing the categories of deconstruction, supplement, play, and repetition. His analysis falters with the *Critique of Dialectical Reason* where he gets bogged down in the trivial problem of the relation of *Search For a Method* to the main body of the text. Also he falsely indicts Sartre as a Cartesian for his use of "clear and distinct ideas." Finally, LaCapra succumbs to misreading the category of the fused group. He takes its position in the cycle of Sartre's categories as a token of its place in history,

whereas for Sartre the fused group is cyclical only in the analytical sense.

LaCapra's purpose is to wed Sartre's radical politics with a redefined notion of dialectical totalization, one informed by supplementarity. While impressive in the clarity of its use of Derrida, *A Preface to Sartre* is as problematic in its thesis as the object it seeks to deconstruct. If the "dialectical totalization of existential Marxism" is too closed, too progressivist, too unified for an "open," contestatory politics, as LaCapra claims, the choice of a Derrideanized Sartre leaves in question the relation of the text to life. At the beginning of the book LaCapra resorts to a new kind of intellectual history that consists of an endless dialogue with texts. By the end of the book a different, far more political, path is invoked. This tension within the book is not explored. Certainly, the strength of Derridean interpretation rests with textual analysis; the union with radical politics is at best a hope and at worst an illusion.

This contradiction has its roots in LaCapra's presentation of the *Critique*. Against LaCapra's view, I see the *Critique* precisely as the groundwork for an open dialectic, one that refutes orthodox Marxism by insisting on mediations between base and superstructure and by allowing for the "supplements" of analytical reason (for instance, structuralism). In addition, the *Critique* explores the relation between commitment and history, life and text, in a manner far more subtle than that in Derrida. In the end LaCapra reduces the advances in the *Critique* to earlier positions held by Sartre, those which are indeed flawed by the traditional humanism that Derrideans find repugnant. Hence, LaCapra's transcendence of the Sartrean dialectic, while open to the applause of those who eagerly consume the new fashion of Derrida, is not as complete as he thinks. The problem is that LaCapra places Sartre's project closer to existentialism than to Marxism.

Nonetheless, LaCapra offers a sensitive and undogmatic reading of Sartre. His book should be read, studied, and criticized, as he himself would wish. It is a far more worthy achievement than either a simple acceptance of Sartre or a monochromatic rejection of him.

MARK POSTER
University of California,
Irvine

PEDRO R. DE CAMPOMANES. *Dictamen fiscal de expulsión de los jesuitas de España (1766-1767)*. Edited by JORGE CEJUDO and TEOFANES EGIDO. (Publicaciones de la Fundación Universitaria Española. Documentos históricos, number 7.) Madrid: Fundación Universitaria Española. 1977. Pp. 224.

Possibly no event of Spanish history has aroused more heated polemics than the expulsion of the Jesuits, carried out in 1767. New light on this old controversy, coming from documents thought long since perished, therefore constitutes an irresistible historical treat.

In December 1766 the fiscal of the Council of Castile, Don Pedro Rodríguez de Campomanes, signed a voluminous *Dictamen*, drawn from the even more bulky *Pesquisa secreta*, to find the true perpetrators of the famous *Motín de Esquilache*. In subsequent years both documents disappeared, and the absence of these and other records contributed not a little to the free play of passion and ideology that afterward prevailed upon this topic.

Miraculously, these and other papers of Campomanes were preserved by his family and are now housed in the *Archivo Campomanes* in Madrid. In this edition Jorge Cejudo and Teofanes Egido have provided the text of the *Dictamen* and useful editorial notes. A long scholarly introduction places the document in its historical setting and critically reviews recent historical writing on the subject.

The editors see the *Dictamen* as a powerful and unrestrained attack on the "essence, presence, and existence" of the Jesuits in Spain (p. 9), based on facts that, although not always clear to the modern historian, were nonetheless accepted by Campomanes and those he sought to influence. Based on the *Pesquisa secreta*, the anti-Jesuit arguments of the French *Parlements* and the Marques de Pombal, and on the virulent anti-Jesuit tradition prevalent in some sections of Spanish society, the fiscal developed an overwhelming case that convinced king and council to order the expulsion.

The historical situation that emerges from the *Dictamen* and the introduction may be summarized as follows. First, Aranda was not the promoter of the process against the Jesuits, nor was he the major mover of the expulsion; this role belongs to Campomanes. Second, the rising in Madrid differed from the movements that occurred in the provinces. Although impelled by hunger, it was more than a mere bread riot or outburst of popular xenophobia. Some sort of direction is to be seen in it, though the source of that direction remains as unclear as ever. Following Carlos Corona and Vicente Rodríguez Casado, the editors argue that the rising did have political significance. If not actually plotted and carried out by an aristocratic cabal that sought to reverse Charles III's policy of promoting the despised *golillas*, the *Motín* would have had that effect had it succeeded; and it fits a pattern of vain attempts to restore the political position of the grandee nobility that runs through the years after the advent of the Bourbons. In some degree, moreover, this confrontation of office-seekers was a confrontation of classes. Third, the Jesu-

its had the immense disadvantage of their supposed historical record—responsibility for the Portuguese rising of 1640, implication in the fall of Charles II of England, resistance to the Anglo-Portuguese boundary settlement of 1750, and so on. Finally, the accumulated enmity toward the Jesuits worked against them. Franciscans, Dominicans, Carmelites, the so-called Jansenists—all had some doctrinal or organizational reason either to remain neutral or to support the decision to expel the Society.

The authors are to be congratulated on a significant achievement; their work clarifies an important and controversial area of Spanish history.

GEORGE M. ADDY
Brigham Young University

JORDI MALUQUER DE MOTES BERNET. *El socialismo en España, 1833-1868*. Barcelona: Editorial Crítica. 1977. Pp. 408.

This is a major study of the origins of Spanish socialism, of the five decades from 1820 to 1870 during which political writers and activists sought an alternative to the model of liberalism institutionalized on the Iberian peninsula. Historians have traditionally glossed over this period in their hurry to reach the First International, an institution and an ambience with which they are far more comfortable. Jordi Maluquer offers new documentation from archives in three countries to replace traditional shibboleths and, of greater significance, a new conceptualization of this phase of Spanish socialism. He deliberately eschews the term utopia in order to demonstrate the viability as well as the historical importance of the Spanish disciples of Saint-Simon, Fourier, and Cabet. He presents these ideas successfully, in my opinion, not only as a coherent criticism of the social relations created by Spain's liberals but as a model for a different social organization and a strategy for transforming the existing agrarian capitalist society.

Although Maluquer's orientation is clearly Marxian, it is in a scholar's format. Thus, while he rejects any concept of ideas as abstractly conceived and transmitted from one group of intellectuals to another, he likewise rejects the reductionist argument that ideas are but the product of material conditions. Instead he posits Spanish "utopian" socialism as part of a West European desire for justice that had not been fulfilled by the French Revolution and as a realistic appraisal of concrete injustices and possible solutions in the Spanish society in which it developed. As a historian, Maluquer is concerned with the impact of these ideas on Marxism and anarchism in the late nineteenth century for the obvious reason that, if they had had

no impact, they would be of interest merely to those studying the bypaths of history. He is, however, studiously careful not to project backward, seeking precedents and auguries of "scientific" socialism. Rather, he studies "utopian" socialism as the product of its time, an autonomous movement breaking off from classical liberalism, working with and yet distinct from radical republicanism and the labor movement. It failed, Maluquer argues, not because of the ideas in themselves nor the ineptitude of its proponents but because socioeconomic conditions were such that potential supporters were too few in number and too divided by conflicting interests to constitute a viable agency of change. But the ideology of republicanism would have been far different, and the First International would not have had so immediate and widespread an appeal, had it not been for this band whose form of socialism was tried and found wanting.

Maluquer's writing, more pedestrian than not, serves well the type of social history espoused by the students of Josep Fontana and Jordi Nadal: archival documentation to trace ideas as they move from books to newspapers to political action.

One limitation is the detachment of this socialist movement from the economic and social developments described in an excellent introductory chapter and in a far less effective epilogue on the transition to the First International. This is undoubtedly the result of Maluquer's decision to divide his dissertation, reserving for a second volume the material on the labor movement from 1820 to 1870. It would have been better to wait until all three elements ("utopian" socialism, republicanism, and the labor movement) could be integrated in one major work. But in Spain as in the United States, young scholars must publish rapidly. This is, however, a minor demurral about an important book.

JOAN CONNELLY ULLMAN
University of Washington

A. E. KERSTEN and A. F. MANNING, editors. *Documenten betreffende de buitenlandse politiek van Nederland, 1919-1945* [Documents Concerning the Foreign Policy of the Netherlands, 1919-45]. Period C, 1940-1945. Part II, *November 1, 1940-May 31, 1941*. (Rijks Geschiedkundige Publicatien, Major Series, number 160.) 's-Gravenhage: Martinus Nijhoff. 1977. Pp. lxxxv, 635. f. 77.50.

J. WOLTRING, editor. *Documenten betreffende de buitenlandse politiek van Nederland, 1919-1945* [Documents Concerning the Foreign Policy of the Netherlands, 1919-45]. Period A, 1919-1930. Part II, *July 1, 1920-August 31, 1921*. (Rijks Geschiedkundige Publicatien, Major Series, number 162.) 's-Gravenhage: Martinus Nijhoff. 1977. Pp. xcix, 649.

These two massive volumes are part of a continuing project on which planning began a generation ago: to publish a definitive collection of documents relating to Netherlands' foreign policy since the mid-nineteenth century. The initial volume, covering the year 1848, appeared in 1972, and since then publication has moved ahead rapidly. Attractive in format, well edited, and as complete as possible, the series has filled in many small nooks and crannies of the historical record while providing no surprises.

The two documentary sequences included here cover periods (1920-21 and 1940-41) that are quite different in tempo. In the first period, the Netherlands was at peace, politics were serene, the Calvinist-Catholic coalition had a comfortable parliamentary margin, and the Foreign Office faced no crises. There is ample space to deal in careful fashion with such familiar debates as that regarding the exact demarcation of the Dutch-Belgian boundary in disputed borderland waters and the similar argument with Germany as to the exact boundary in the Ems estuary (which line of sandbanks should be used?). And there are other issues discussed at length, such as the knotty question of the Pacific cable network (in which there were heavy Dutch investments) and the junction point at Yap, or the perplexing problem of finding a balance between Standard Oil's request for exploration rights in the East Indies and the rather special position of Royal Dutch. Sometimes an old concern appears in new form: trade relations with Russia must be re-examined, now that the Communists have taken over. But the pace is comfortable and leisurely.

Twenty years later, Pieter Gerbrandy headed a government in wartime, in exile, headquartered in London. France had fallen, the Battle of Britain raged, and the Foreign Office deliberated while air-raid sirens wailed. Dispatches and memoranda were often urgent. In the midst of its many worries, the focus of Foreign Office concern was the Netherlands East Indies. Behind the continued interchange of polite diplomatic notes with Japan, there was rapidly rising suspicion, distrust, and alarm regarding Japanese intentions. Should the Gerbrandy government move to Batavia? Should it begin military consultations with Great Britain and the United States? Secret meetings were arranged. The Foreign Office tried with increasing haste to gain pledges of military help in case the Indies were invaded. As the seven-month period covered by the documents ended, there was a strong sense of impending crisis in the Pacific. Needless to say, such fears were not unfounded: Pearl Harbor occurred within a few months.

Both sets of documents are admirably overseen by their editors. The arrangement in each volume

is chronological, day by day. There are extensive footnotes, complete Foreign Office personnel lists, a very complete set of indexes, and a brief summary of each document in English. In short, two excellent additions to an excellent series.

DIRK JELLEMA
Calvin College

KERSTIN MOBERG. *Från tjänstehjon till hembitråde: En kvinnlig låglönegrupp i den fackliga kampen, 1903-1946* [From Household Drudge to Domestic Servant: The Struggles of a Group of Poorly Paid Women Trade Unionists, 1903-46]. Summary in English. (Acta Universitatis Upsaliensis. Studia Historica Upsaliensia, number 101.) Uppsala: Historiska Institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell, Stockholm. 1978. Pp. 256. 83.00 KR.

A precise definition of scope may help to clarify at the outset the contributions and limitations of this recent Swedish doctoral dissertation. The core of Kerstin Moberg's data lies in the records of the Stockholm Maidservants' Association, founded in 1904, which changed its name in 1917 to the Stockholm Domestic Servants' Association (*Stockholms Hembiträdesförening*, or HBF). At their peak around 1942, all of Sweden's domestic servants' associations claimed some five hundred members out of 140,000 women in domestic service jobs in cities and towns, and thus the organized movement can in no way be taken to represent the entire social group.

Moberg's purpose is to describe the history of HBF primarily on the basis of materials now in the Labor Movement Archives at Stockholm, to identify shifting goals and strategies from 1903 to 1946, and to determine the extent to which Sweden's labor unions and Social Democratic political organizations lent support to the domestic servants' movement. Aside from an interesting chapter (chap. 3) that summarizes survey results collected in 1934, there is no systematic analysis of women in Swedish domestic service during this period, nor is the work cast as a study in the status of women, although Moberg does note ways in which the experience of maidservants resembled or diverged from that of other women workers. Finally, this is not a study of a conventional "low wage" group in trade union strategy: most maidservants did not perceive wages to be an acute problem. Decent housing, social status, and guaranteed time off were the most regularly articulated demands.

The central strategic problem for HBF and for the other organizations that looked to HBF for national leadership was the extent to which the association could come to function as a trade

union. After early years as a "liberal" organization of both employers and employees, HBF's predecessor affiliated with the Stockholm trade union council in 1912, but until the mid-1930s Sweden's national trade union federation, Landsorganisationen i Sverige (LO), gave very little active support. On its own initiative in 1924, HBF tried to persuade its labor market counterpart, the National Association of Swedish Housewives, to enter into formal collective, or even individual, bargaining and agreements concerning working conditions, but the housewives refused to adopt the functions of an employers' federation. Together with the fact of a highly transient workforce in domestic service and a lack of political consciousness among young maidservants, the resistance of housewives prevented HBF from operating as a trade union, and this in turn made membership recruitment extremely difficult. Moberg finds the evidence too slim to indicate whether HBF might have become a successful union with stronger LO support at critical moments. In the 1930s and 1940s LO did provide money and personnel for reasons of employment policy, but by that time social and technological changes were starting to eliminate domestic service as a major occupation on the Swedish scene.

Frustrated in its trade union goals, HBF turned after 1930 to legislative action through the Social Democratic Party. The timing was fortunate as Socialists hoped simultaneously to win voters and to counteract Communist agitation within workers' organizations. Due to procedural delays and wartime circumstances, the Domestic Servants Act was not passed until 1944, when the shortage of applicants for domestic service jobs had already resolved the most acute problems for women in this group.

Moberg's decision to focus her study narrowly on HBF and its archives has some disadvantages insofar as several important questions remain unexplored. One of the most important concerns the factors underlying LO's lack of interest and support in the crucial early decades. Was this yet another sign of masculine blindness to the needs of women workers, or was it a practical decision not to invest scarce resources in an unusually difficult organizing campaign? The question can only be clarified by much broader research into attitudes toward women among Swedish labor movement groups and leaders, a task recently addressed by Docent Gunnar Qvist in several publications. Meanwhile, Moberg's dissertation serves to retrieve a limited range of neglected information for future use. The English summary is satisfactory, but there is no index.

ROBERTA G. SELLECK
Harvard University Library

HEIKKI KIRKINEN. *Karjala taistelukuultänä: Karjala idän ja lännen välissä*, [Karelia as a Field of Battle: Karelia between East and West], part 2. (Historiallisia tutkimuksia, number 99.) Summary in English. Helsinki: Suomen Historiallinen Seura. 1976. Pp. 376.

The publication of this third volume of Heikki Kirkinen's study of Russian Karelia during the Middle Ages and the Renaissance marks a turning point. For the first time, readers have access to a balanced, modern account (using Russian as well as Western sources) that covers the rich, almost mythical past of the disputed Karelian borderland between two worlds. Volume one (whose title in English is *Karelia in the Eastern Cultural Sphere*) described the emergence of the Karelians as a people and their gradual attraction into the cultural and political sphere of Novgorod and the Eastern Church. The volume under review and the previous volume (in English, *Karelia between East and West: Russian Karelia during the Renaissance, 1478-1617*) cover the period from 1478 to 1617, that is, from the point when political control of Russian Karelia shifted from Novgorod to Moscow down to the Peace of Stolbova; volume two dealt with social, economic, and cultural history; volume three deals with political events.

Nominally limited to Russian Karelia (excluding Finnish Karelia and Savo [Tavastland]), the volumes raise serious questions that may force a reinterpretation of many aspects of the cultural and political relationship between Swedish-Finnish interests and the East. Many of Kirkinen's findings would have been, and were, treated as radical and polemical a few years ago. Exhaustively documented and based upon large amounts of new evidence, Kirkinen's interpretation marks a definitive break with the brilliant but heavy hand in which, for half a century, the late J. Jaakkola and his school held the history of the Eastern relations of medieval and Renaissance Finland.

The recurring theme of "Russian pressure" and Finland's geographic role as a buffer against Russian expansion—repeated in almost every Western history of this period—is revealed as little more than a rationalization of Western expansion. Kirkinen's detailed analysis of border happenings in volume three brings the whole East-West conflict down out of the clouds of ideology and identifies the strong role played at the local level by the Karelians rather than by distant rulers. Presumed until recently to be only vaguely defined, the western portion of the 1321 frontier at the Gulf of Bothnia was drawn meticulously in a place that guaranteed the Karelians access by canoe to a colony of beavers!

In the space available it is impossible to list the

many important differences of fact and interpretation between this and previously authoritative works. One of the more important, in addition to the generally "defensive" interpretation of actions by Novgorod and Moscow during the period of Swedish expansion, is the re-examination in volume two of the disputed origins of the more important themes of epic folklore (known to most non-Finns in relation to the *Kalevala*). Kirkinen dismantles the views advanced in the 1920s by Kaarle Krohn and J. Jaakkola that the Kalevala themes (including the epic portions) originated in western Finland, disappeared there, but were somehow transmitted to the East, where they survived. Although the treatment includes thematic analysis of folklore, the weight of the argument derives from historical evidence, particularly the chronology of diffusion of folklore and the evidence of Karelian shamanic traditions in a broad geographic area.

Perhaps the greatest revelation of this series is the social, cultural, and economic history of Russian Karelia. No existing work in any way approaches the richness of detail of volume two. Among a population estimated at one hundred thousand in the 1500s (one-third that of Finland at the time), Kirkinen shows a prodigious rise of material and spiritual life—more than sixty monasteries, widespread literacy in certain classes, prosperous traders, vigorous early industry, and even the beginning of literature in the Karelian language in the mid-1500s (contemporaneous with the efforts of Agricola in Finland).

The major weakness of the series is structural. Planned as an integral, separate work, volume one overlaps to some extent volumes two and three, particularly in the portions on cultural life. Had the three been published as a unified whole, volume one would presumably have eliminated this overlap and replaced it with a more thorough consideration of political history in the period prior to 1467. The chapter in volume three on border problems between 1323 and 1478 is extremely useful but could well be set against an equally detailed treatment of the broader history of the period. That volume one is, apparently, relatively "weaker" than the other two volumes is due in large measure to the extraordinary difficulties in dealing with this period; the weakness is inherent in the subject rather than the author's treatment. An exception is the treatment of the question of language relationship in volume one, namely whether Karelian is related more to Vepsen than to western Finnish dialects; in the light of the importance accorded to this issue in many parts of the literature, the arguments of linguists such as Nirvi should be weighed in detail against historical evidence. By contrast, I consider that the most controversial thesis for in-

ternational politics is solidly grounded in new evidence, namely "that the Karelians' own interests were usually in the foreground of border questions and that Russian central authorities generally intervened in matters on the basis of the initiatives and desires of the Karelians and only more rarely on their own initiative. Russia's fundamental border problems were more to the south in the Baltic and on the borders of Poland-Lithuania" (Volume three, p. 289).

If, as one fervently hopes, these works are translated into a more accessible language, a revision of volume one should remedy the structural problem referred to above. As it stands, Kirkinen's work represents the most important contribution to Karelian history since the publication in 1915 of Voionmaa's pioneer history of the Karelian people.

STACY CHURCHILL

Ontario Institute for Studies in Education and
University of Toronto

HANNAH RABE. *Das Problem Leibeigenschaft: Eine Untersuchung über die Anfänge einer Ideologisierung und des verfassungsrechtlichen Wandels von Freiheit und Eigentum im deutschen Bauernkrieg*. (Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte. Beihefte, number 64.) Wiesbaden: Franz Steiner Verlag. 1977. Pp. xi, 128. DM 28.

This exceedingly dense, compact study proposes a unique interpretation of late medieval developments in the old, traditional legal relationships concerning personal freedom and property that figure prominently in understanding the German Peasants' Revolt of 1525. The received interpretation, as represented by Hannah Rabe, declares that the revolt was a rejection of processes by which the landlords had come to monopolize the land and, through serfdom, had locked the landowners into absolute loss of freedom. This historical picture is maintained as the essence and content of rulership by the old European nobility.

To refute the tradition, the author undermines its key element. She insists that an evolutionary process which deprived landworkers of property and personal rights vested in them through membership in the community cannot be validated. Yet on this hypothesis rests the whole of the old view. Next, the author conducts a minute search for the meaning of the concept, *Leibeigenschaft* (serfdom). Her analyses lead her to conclude that a new and dreaded meaning had evolved alongside the term's traditional connotation of honorable and positive relationships in the personal freedom of an individual. But the new meaning actually was a response to changes regarding *Eigenschaft*, the whole realm of property rights and relationships as structured

within the lords' lesser jurisdictional powers. The peasants, perceiving or fearing bondage, mistakenly applied the expression, *Leibeigenschaft*, to rapid change being forced in *Eigenschaft*. Finally, the author undertakes a case for changing concepts in basic law and normative thinking. Pressure from the lords cumulatively undermined the old order of property and personal rights. This complex had given existence to the peasants in the past; dismantling it was akin to denaturing and dispossessing the landworkers.

In fine, that which the peasants in 1525 called *Leibeigenschaft* was a reality but a misnomer. They were referring to a process in *Eigenschaft*, real or feared, the end of which was to change the old communal order by stripping the peasants of traditional precedents in their personal and material obligations that actually had given them person and property. This process resulted in a peasant much like the self-dependent laborer under capitalism. Converting communal individuals into self-support individuals by removing them from the system, thus, contrasts sharply with the received interpretation that the old system provided the lord with monopoly control over persons and property. Seen correctly, the old system, which was a self-existent component of the constitution of old-European common life, has little resemblance to the modern, constitutionally protected legal system of private property. In both instances, however, mastery of the means of production and the productive capacity of the worker is the end result.

To this reviewer, what it all seems to mean is that the peasants' revolt is relieved of having to be a progressive milemarker in the emergence of the bourgeoisie. If the constitutional and conceptual changes underway looked toward the modern individual laborer, then the feudal lords were doing the work of the capitalists and the precapitalist revolution is not necessary. All of the contradictions in the traditional and the contemporary socialist interpretations may be resolved. Indeed, there is a high degree of similarity between the fullness of rights in the old, estates-based property system of the medieval peasant and the communal and common rights found in the modern socialist lands.

All the foregoing rests upon the unsure assertion that this reviewer has understood the author, for the capacity of the German language to permit monstrous constructions and impenetrable accretions, compounding words that arrive at obfuscation in the misguided name of refined clarity and meaning, has resulted at the author's hands in a linguistic horror of classic degree. Add to that the necessity of being alert to Marxian mirror-reverse sleight-of-mind and reading the book becomes an exercise in control over frustration and rage. This

work may be important, but this reviewer is not sure.

KYLE C. SESSIONS
Illinois State University

THOMAS A. BRADY, JR. *Ruling Class, Regime and Reformation at Strasbourg, 1520-1555*. (Studies in Medieval and Reformation Thought, number 22.) Leiden: E. J. Brill. 1978. Pp. xxi, 458, f 124.

This is an important book in many respects. On one level it is a thoroughly researched prosopography of Strasbourg's ruling oligarchy, ca. 1520-ca. 1555, the data for which Thomas A. Brady, Jr., presents in appendixes that will be invaluable to other scholars. On another level, it is a structural analysis of the government's social base—at once feudal and bourgeois, noble and mercantile—and a study of its reactions to the crises of the Peasants' War and the defeat of the Schmalkaldic League. Precisely because Strasbourg's ruling class consisted of noble rentiers and big merchants on the one hand, and the artisan, shopkeeper, and guildsman on the other, it was able to hold firm in 1524-25 but split into two hostile factions in 1547-48.

Although more precisely argued than heretofore, Brady's interpretation of these events will come as no surprise to specialists. The novel element in this work occurs at yet another level. In brief, he sharpens Moeller's interpretation of the urban Reformation by arguing that its real home was with the shopkeepers and guildsmen who did not have far-flung interests but did participate in the regime, at least at its periphery. These were the people whose commitment to the values of the Reformation was so strong that they consistently favored resistance to the emperor, even at the cost of life itself. By contrast, the aristocracy found it could afford to yield religious reform in the 1520s but literally deserted the Reformation—and the city—when Charles V's victories threatened their real interests.

Herein lies the most important challenge of the book. The social historian with an eye to broad correlations will find Brady's analysis of the popular Reformation thoroughly convincing. Students of the human spirit will note that some nobles used almost the same language as did artisans in explaining why they chose resistance and will object to the easy but unproved assertion that Lutheranism was the more feudal and Zwinglianism the more radical of the new religions. Some may indeed be inclined to set the argument on its head and draw from it persuasive evidence that a movement of values and ideas, such as the Reformation, had sufficient autonomy and force to throw into

disarray one of the most stable regimes of early modern Europe.

Consequently, this book is also challenging on the level of its approach. Brady rightly begins with forty-seven pages of prolegomena in which he properly criticizes the main trends of Reformation historiography and clarifies his own approach to the study of social classes in the late feudal age. Both discussions will provoke heated argument, if only because Marx appears to be the exclusive, clanking ghost of Christmas past, come to turn Reformation historians from their rigidly dogmatic present. The critique is accurate; the solution appears to substitute new shackles for the old.

In sum, what Brady has to say is important, far beyond the confines of Strasbourg's aristocracy. Lively debate should follow.

JAMES M. KITTELSON
Ohio State University

JAN ŠOKTA *et al.* *Geschichte der Sorben*. Volume 2, *Von 1789 bis 1917*. (Akademie der Wissenschaften der DDR, Schriftenreihe des Instituts für Sorbische Volksforschung in Bautzen.) Bautzen: VEB Domowina-Verlag. 1974. Pp. 328.

MARTIN KASPER. *Geschichte der Sorben*. Volume 3, *Von 1917 bis 1945*. (Akademie der Wissenschaften der DDR, Schriftenreihe des Instituts für Sorbische Volksforschung in Bautzen.) Bautzen: VEB Domowina-Verlag. 1976. Pp. 219.

Numbering some two hundred and fifty thousand in the early nineteenth century, the Sorbs (also Wends or Lusatians) constituted a majority of the population along the northern border of Saxony and in adjoining districts in the Prussian provinces of Saxony, Brandenburg, and Silesia. Now numbering fewer than one hundred thousand they reside primarily in the districts of Cottbus and Dresden in East Germany. This general history, published by a branch of the official East German Academy of Sciences devoted to Sorbian studies, presents the results of extensive research in condensed and semi-popularized form. Volume one (*AHR*, 83 [1978]: 743) carried the story to the late eighteenth century. Volumes two and three are dominated by two parallel themes, the history of the Sorb national movement and the impact of economic development on the Sorb people.

The sections on economic, social, and cultural history are somewhat disappointing in their thinness and lack of conceptual clarity. Too often the authors assume that changes in the Sorb districts must have proceeded in accordance with a general model of the development of capitalism, and they substitute that assumption for close analysis of local experience. Surprisingly, the authors do not

consider demographic patterns among the Sorbs, and the Sorbs' slow and hesitant urbanization remains largely unexplored. Most disappointing, perhaps, is the failure to treat Sorb culture in greater detail. The authors assert repeatedly that the Sorbs remained fundamentally democratic and humanistic but do not support their assertion with analysis of institutions, customs, or the extensive Sorb tradition of song and poetry.

The sections recounting the history of the Sorb national movement and its contacts with surrounding German and Slav nationalisms are more rewarding. Demands by national minorities for autonomy have been common enough, of course, but the extraordinary persistence of the Sorbs' demands over a century and a half suggests that the threshold size for a viable "nation" may be very small indeed. The Sorbs passed through each stage of nineteenth-century nationalism yet managed to give each a distinctive flavor. A "Wendisches Seminar" was established at the University of Prague by the Jesuits in 1706 to train theology students from Lusatia. After the Jesuits' expulsion in 1773, the seminar became a center of Enlightenment thought and of Slavic studies under Josef Dubrovský. The close connection of the Sorb and Czech nationalist movements continued; and Prague trained most leaders of Sorb nationalism until 1921, when the Seminar was closed by the new Bishop of Meissen, a German and self-proclaimed opponent of Slavic culture.

Throughout the nineteenth century, members of the small, university-trained elite dominated the Sorb national movement, though their direct contact with local organizations and their efforts to foster Sorb music and literature kept them in relatively close contact with their followers. The rapid proliferation of specialized Sorb organizations before the First World War paralleled developments elsewhere in Germany, as did the foundation in 1912 of the Domowina, or Union of Sorb Associations. In 1919 Domowina leader Arnošt Bart traveled (using a Czech passport) to Versailles, where he appealed personally but in vain to Allied leaders for Sorb self-determination. He was arrested upon his return to Germany.

The obverse of the Sorb national movement is the consistent policy of Germanization pursued by the Prussian, imperial, and Weimar governments. A "Lusatian patriot" argued in 1805 that suppression of the Sorbs' language would "bring them closer to the Germans, raise their culture." Until 1933 the struggle centered primarily on language instruction and use. The Nazis lumped the Sorbs together with other Slavs, and Himmler's plan to resettle them in the East as slave labor was postponed "until the end of the war" only because of the worsening military situation in 1942.

Serious conceptual problems, however, undermine the analysis of Sorb nationalism. The authors' overly deterministic version of Marxism actually leads them away from Marx and back toward Herder. The authors conceive of "nations" as unitary, ideal entities, each of which has been assigned a particular role in world history. This linear view precludes consideration of any dialectical development, either within the movement or in relation to the surrounding world. The important divisions between Northern and Southern Sorbs, who speak distinctly different dialects, are therefore treated in a vague and unsatisfactory manner. The authors are merely impatient with disputes among Sorb nationalists, and they largely ignore potentially embarrassing topics such as Sorb support for conservative parties. Reichstag election results show that with only two exceptions conservatives triumphed in the Cottbus-Spremburg and Rothenburg-Hoyerswerda electoral districts, and that Bautzen-Kamenitz consistently returned anti-Semites in the imperial period. Later, at least some Sorbs joined the Nazis (3:135-37).

Throughout the nineteenth and twentieth centuries, definitions of the Sorb nation have reflected the interests of the speaker, and these volumes are no exception. The authors state that the "historical mission" of the Polish, Danish, and French minorities in Germany was to struggle for reunification with their respective nations. "The historical mission of the Sorb people, in contrast, was to act on behalf of . . . a lasting, friendly life together with the German people." Further, this purpose has now been realized in "the national equality of rights and full flowering of Sorb culture in the German Democratic Republic" (2:130, 162). Perhaps. But few of the Sorb nationalists considered in these volumes would have agreed.

FRANK B. TIPTON, JR.
University of California,
Riverside

BERND WUNDER. *Privilegierung und Disziplinierung: Die Entstehung des Berufsbeamtentums in Bayern und Württemberg (1780-1825)*. (Studien zur Modernen Geschichte, number 21.) Munich: R. Oldenbourg Verlag. 1978. Pp. 349. DM 95.

This volume is a study in the origins of German civil service law (*Beamtenrecht*). The term *Beamtenrecht* pertains to the conditions of employment for higher civil servants. These conditions include lifelong tenure, provisions for old age and invalidity (including the right to a pension and to financial security for the civil servant's dependents), removal from one's position only if the individual abuses it or conducts a life incommensurate with

the performance of the duties and the dignity of the office, the requirement that civil servants commit their entire person and abilities to the execution of their duties and that they perform them with appropriate discretion, and the establishment of a special administrative judicial system to discipline them.

Bernd Wunder regards as uniquely German the concept of a higher professional civil service with such regulations and perquisites. Although this conclusion is doubtful, the author presents a cogent analysis of the development of civil service law in Bavaria and Württemberg in the late eighteenth and early nineteenth centuries. His *pièce de résistance* is a discussion of the governmental program of Count Montgelas, Bavaria's leading minister from 1799 to 1817. Wunder draws on archival sources in Munich and Stuttgart to argue that a tenured, university-educated civil service was created as the principal tool of reforming absolutism: the rulers of the South German states introduced reforms to meet the external challenges of the era of the French Revolution; these reforms were implemented by a higher state bureaucracy whose allegiance was gained by bestowing privileges on the official and his family. Wunder suggests that the small stratum of *Akademiker*, the highly educated, were won to the side of the prince through these privileges, which gave members of their social group preference in obtaining secure employment: the prince gained reliable support against the nobility and, in Württemberg, against the estates as well. Wunder finds in the privileges of the nobleman the model for the privileges of the higher civil servants: through these privileges the higher civil service acquired a position outside civil society and became a self-recruiting body that resembled a social caste, not a social class.

Wunder describes his work as a corrective to most of the literature on the development of the state bureaucracy in Germany; his point is exaggerated. Of course Gustav Schmoller, Otto Hintze, and many other pro-Prussian scholars sought to demonstrate that the state bureaucracy was highly developed in Prussia before the French Revolution; their admiration for Prussian institutions led them to overlook the maturation of the civil service in other German states. But Wunder's research cannot answer the question of priority, which would require a parallel study of the Prussian bureaucracy.

He is on firmer ground when he stresses that the special privileges and obligations of the state servant must be understood in historical context. Much of the German literature on bureaucracy depicts these privileges as necessary functional concomitants of any professional administration. This literature is written mainly by *Juristen*—by

teachers of future state administrators and their pupils. Their works are generally deficient in historical analysis, but Wunder has spent so much time with these writings that he overestimates their currency. He also implies that today's West German civil service law is antiquated—a suggestion certain to ruffle many feathers. All in all, Wunder's book is a solid monograph to add to the shelf of specialized literature on the development of bureaucracy in Central Europe.

WALTER STRUVE
City College,
City University of New York

PAUL R. SWEET. *Wilhelm von Humboldt: A Biography*. Volume 1, 1767–1808. Columbus: Ohio State University Press. 1978. Pp. xiv, 307. \$18.50.

This first volume of a planned two-volume biography treats Humboldt's childhood and student years, his brief career in the Prussian department of justice, his marriage, the early esthetic and political writings, his years in Jena and Paris, the two journeys to Spain (important in advancing his study of comparative linguistics), and finally his return to government service as Prussian resident in Rome from 1802 to 1808, which proved to be an essential step toward the senior political appointments of the coming years. Humboldt's social environment and the intellectual and political influences to which he responded are analyzed with as much care as the growth of a many-sided, if occasionally contradictory, talent. Paul R. Sweet appears not to have discovered new material, but a mastery of the very extensive primary and secondary literature is reflected in his well-organized and clearly written work. Presumably a bibliography and index will be included in the second volume, which is announced for the summer of 1979.

An earlier German biographer, Friedrich Schaffstein, noted in his *Wilhelm von Humboldt* that biography seeks to separate the historically significant elements of the subject from the purely accidental realities that envelop his physical and emotional existence. Determining where to draw the dividing line has always presented special problems in Humboldt's case, because his personal and emotional life seems to impinge on his theories and politics with extraordinary strength. Humboldt's self-revelations are so abundant that the difficulty may simply be that we know him better than most of his contemporaries. And yet, with Humboldt the interaction between the private and public man is unusually intense and fluid—far more so than with his colleague and eventual antagonist Hardenberg, who knew, for instance, how to isolate his strong eroticism from his administrative

and political work throughout much of his long career in government. In a famous revisionist biography of the 1920s, Siegfried Kaehler explained Humboldt's political failure after 1815 largely in terms of psychological weaknesses, especially of sensuality; Kaehler argued that its sadistic component masked a basic passivity. While providing an effective organizing principle for his book and deepening our understanding of Humboldt, Kaehler constructed his hypothesis far too narrowly to support such conclusions. Paul Sweet finds Kaehler's argument unpersuasive, but he does not oppose it with a comprehensive hypothesis of his own. His analysis appears to take its structure from the events and ideas that made up what might be called the preparatory phase of Humboldt's life, rather than from a basic theme that the historian has discovered in these phenomena or that he has decided to impose on them. It is too early to say whether his work will lead to important reinterpretations of Humboldt's personality, writings, or politics; but there can be no doubt that the English-speaking world now possesses a reliable and far-ranging account, which incorporates the most recent scholarship, of the first four decades of Wilhelm von Humboldt's life.

PETER PARET
Stanford University

MATTI VIHARI. *Die Krise der "historistischen" Geschichtsschreibung und die Geschichtsmethodologie Karl Lamprechts*. (Annales Academiae Scientiarum Fennicae. Dissertationes Humanarum Litterarum, number 13.) Helsinki: Suomalainen Tiedekatemia. 1977. Pp. 483. 120.00 mk.

Karl Lamprecht is surely the most fascinating character of late-nineteenth-century German historiography. He was the black sheep in the professional historians' fold, rejecting the primacy of politics in favor of the economic, social, and cultural facets of the past. He was open to "Western" positivistic ideas and claimed to have found empirical laws of historical development. Lamprecht's deviations from the traditional mode appear all the more attractive in the light of the unwitting complicity of traditional German historical thought in the disasters of the twentieth century. Thus the time is ripe for a good book on Lamprecht—one that clearly analyzes his diverse and often confusing work and sets him firmly into the context of nineteenth-century German intellectual life.

Matti Viikari's book fulfills this need only partially. It is his intention "to explain and describe the intellectual and social history of the origin and destruction of the Karl Lamprecht phenomenon" (p. 10, my emphasis). For this reason, he sur-

rounds the main 300-page core of material on Lamprecht with 150 pages on various topics, including the historicist tradition from Leopold von Ranke through Max Lenz, the social and economic development of late nineteenth-century Germany, the growing conservatism of the universities, and Heinrich Rickert's Neo-Kantian justification of historicist methodology. This material is not, unfortunately, integrated successfully with the much longer treatment of Lamprecht himself.

The sections on Lamprecht are quite rigorous and build up a comprehensive picture of his theoretical position as well as his view of German historical development. Viikari's judgments here are very balanced, and he does not attempt to mask the many contradictions and arbitrary assertions found in Lamprecht's work. The treatment of the historiographical context of Lamprecht's early work on medieval economic history is especially impressive, as is the discussion of various intellectual influences on the *Deutsche Geschichte*. Viikari's work on Lamprecht is scholarly and professional, marred only by a few technical details such as overuse of long quotations and frequent repetition.

The sections of the book on the historicist tradition, on the other hand, are very general, and little sense of individual variety within the tradition emerges. Viikari treats the historicist side of the *Methodenstreit* sparsely, presumably because its adherents responded to their threatened social status only by automatic repetition of the established canon of the historians' guild. Although I admire the author's attempt to broaden the perspective of his work beyond a single historian, these sections are simply not adequate to bear the weight of his argument.

Viikari's judgments on the historicist tradition are quite harsh—a stance that has been fashionable over the last ten years, and is just to a certain extent, given the historians' role in supporting Germany's nationalistic preoccupation. An added reason for Viikari's condemnation is his Marxism. Especially for Marxists, the great errors of the historicist tradition were its neglect of economic history, its rejection of materialism for "mystical" and "antitheoretical" methods, and its denial that laws of historical development could be found. Lamprecht, on the other hand, scores well in all these departments, and Viikari can therefore see him as a "tragic" figure (p. 454), even though his political stand by 1900 was even more harshly imperialist than that of his conservative foes. Still, Viikari's Marxism is mild and evenhanded. He uses many recent (and often neglected) East German sources on historiography but balances them with Western sources as well. His position only seldom degenerates into clumsy economic deter-

minism (p. 261n), and his Marxist orientation adds interest to his evaluation of Lamprecht.

This book is timely but perhaps too ambitious. The sections on Lamprecht himself are far more useful than the broad framework of "crisis in the historicist tradition" into which Viikari attempts to set them.

CONSTANCE N. GENGEBACH
St. Olaf College

KARLBERNHARD JASPER. *Der Urbanisierungsprozess dargestellt am Beispiel der Stadt Köln*. (Schriften zur Rheinisch-Westfälischen Wirtschaftsgeschichte, number 30.) Cologne: Selbstverlag Rheinisch-Westfälisches Wirtschaftsarchiv zu Köln. 1977. Pp. 306.

In both the United States and Great Britain "urban history" has grown remarkably in the last decade. But in West Germany, despite Wolfgang Köllmann's pioneering efforts, it has barely gotten off the ground. Karlbernhard Jasper's new book must therefore be welcomed as an indication of growing German interest in this important field of research.

Jasper's main intention is to provide an analysis of the "process of urbanization" in late nineteenth-century Germany by focusing in considerable detail on the history of one of its major towns—the industrial and commercial center on the Rhine, Cologne. The author starts by tracing the actual physical expansion of the city from the 1870s to World War I and then moves on to the basic demographic history (birth and death rates, migration, age structure, sex ratios, family size, confessional distribution). This is followed by an investigation of local economic structure, housing, and development of public and private transport and, finally, by a brief consideration of the "material situation of the population."

Jasper leans heavily on the published statistical sources, which is understandable given the insatiable statistical appetites of nineteenth-century German bureaucracies, local governments, chambers of commerce, and voluntary associations that produced mountains of figures. But these figures do not always explain the developments that they describe, and frequently they are silent on important issues. In such cases the answers must be found elsewhere by calculating other statistics from different sources and by drawing on more qualitative forms of evidence. But Jasper seldom does so. To cite one example, he points out that well into the twentieth century Cologne's death rate remained higher than that of several other large industrial cities in the Rhineland and Westphalia (p. 52); but he does not offer any real ex-

planation for this important piece of information. Similarly, when he investigates social stratification, Jasper restricts himself largely to what the published statistics can tell him (p. 99). He makes no attempt, for example, to reconstruct occupational mobility patterns from the city address books or to calculate measures of endogamy and exogamy from manuscript marriage registers.

Some readers, consequently, may feel that much of Jasper's social analysis, like the discussion of the housing question in Cologne in the early twentieth century, as the *Rheinische Zeitung* put it then, remains trapped in the narrow confines of "dead statistical tables" (pp. 144-45). But, fortunately it is just at the point at which Jasper begins an extended investigation of the housing question (pp. 109-92) that his treatment becomes more evenly balanced between quantitative and qualitative approaches and, thus, far more insightful regarding the social and political determinants of urbanization in Germany. Indeed, Jasper's discussion shows quite clearly that the patterns of housing in Cologne were the outcome not of an impersonal and objective "process" but of social conflicts and relationships. The author allows the city to emerge as a human artifact because the statistics are made to take a back seat to discussions of the operation of the local labor market and of the behavior and activities of landlords, renters, industrialists, workers, city government, cooperative building societies, charitable associations, and the local *Arbeitersekretariat*.

Statistical sources like those used by Jasper undoubtedly will continue to provide grist for the mill of German urban historians. But before they rush to use them, historians should first consider whether they share all of the priorities and concerns of the men and women who originally gathered these figures in the nineteenth and early twentieth centuries.

DAVID F. CREW
Columbia University

GEORGE O. KENT. *Bismarck and His Times*. Carbondale: Southern Illinois University Press. 1978. Pp. viii, 184. Cloth \$12.50, paper \$4.95.

Professional historians will find the principal value of George O. Kent's brief biography to reside in the footnotes. The concise text of 131 pages contains no surprise. But the notes carefully dissect the major historiographical debates that have agitated scholars of imperial Germany during the past few decades: the failure of nationalism in 1848, the ensuing constitutional conflict in Prussia, the Hohenzollern candidacy, the unification of the German states, the annexation of Alsace and Lor-

raine, the Kulturkampf, the creation of a European system of alliances, the belated turn to imperialism, and the chancellor's graceless fall from office in 1890. Several of these essays are gems of lucidity that display both Kent's wide reading and his ability to sketch the main lines of controversy with a few firm strokes. Anyone seeking a quick orientation in Bismarckiana would do well to consult Kent's reliable indications.

It is only regrettable that the book's nine chapters show little imprint of the most recent writings that Kent so capably evaluates in his annotations. In conception and execution the narrative follows the conventional form of political biography from birth to death. This serves as a useful *mise au point* of past scholarship. Yet it also suggests that future research will need to free itself from the confinements of the biographical mode. Otherwise, the distance between text and notes is likely to grow still wider.

ALLAN MITCHELL
University of California,
San Diego

RONALD J. ROSS. *Beleaguered Tower: The Dilemma of Political Catholicism in Wilhelmine Germany*. Notre Dame: University of Notre Dame Press. 1976. Pp. xx, 218. \$14.95.

It is gratifying that the neglected subject of German political Catholicism has begun to receive attention from historians in the U.S. This well-documented study concerns a period of crisis for German Catholics and for the Catholic Center Party (*Zentrumspartei*, or "Centrum" in the author's rendition). Despite the title, the book does not cover the entire Wilhelmine era but concentrates on the last ten years before the war; thus, it complements and in no way duplicates the recent monograph by John Zeender on the Center Party from 1890 to 1906. Ronald J. Ross provides an index, statistical tables, and an extensive and very useful bibliography.

Ross examines three aspects of the crisis: the continuing effort to achieve "parity" for German Catholics by the "elimination of discrimination in the laws . . . in government service and the professions and in politics" (p. xv), the conflict within the Center about the nature of the party (the *Zentrumsstreit*), and the controversy over the interdenominational Christian Trade Unions (the *Gewerkschaftsstreit*). Ross uses the term "parity" as a leitmotiv to interrelate these three issues, sometimes to the point of straining the analogy. He describes the party conflict as a struggle for parity of laity with clergy, of West with East, and of Poles with Germans; he describes the union controversy

as a struggle for parity of labor with bourgeois interests. All three issues posed the question of whether to end the separate status of Catholics in Germany or to achieve desired goals within the "tower" of exclusive Catholicism.

In the party conflict, those belonging to the "Bachem orientation," led by a prominent publishing family of the Rhineland, sought to transform the Center into a purely political party and to loosen its ties with the Catholic clergy. Their primary goal was to change the Center's sectarian image in order to facilitate a political alliance with the Conservative Party. The "Berlin orientation," led by Cardinal Kopp of Breslau, wanted the party to remain under close supervision by the clergy. The contest was an uneven one from the start, since the Berlin faction had almost no support within the party membership, and ended with a victory for the Bachemites. The union conflict involved much the same antagonists. Catholic associations for the moral and spiritual welfare of industrial workers had been formed in Germany soon after the publication of the papal encyclical, *Rerum Novarum*, in 1890, but they were no substitute for genuine labor unions, and by 1900 Catholic workers in the industrialized Rhineland had organized a union movement. Although these Christian Trade Unions remained largely Catholic in membership, they were interdenominational in principle and were led by laymen rather than clergy. For this reason, they were opposed by the Berlin-Trier school of thought and the Catholic leadership in Silesia and were disliked by Pope Pius X, who associated them with other undesirable forms of "modernism." The Bachem-Cologne school and the Center Party supported the unions, though without much enthusiasm. The German imperial government favored the Christian Trade Unions, considering them a useful weapon against Social Democracy, and Ross reveals that in 1913 Chancellor Bethmann-Hollweg intervened through the Prussian ambassador to the Vatican to dissuade the pope from forbidding the unions.

Ross has brought together a wealth of important material, but its very richness and density may prove bewildering to readers who do not already possess some familiarity with his subject. One source of possible confusion is Ross's misuse of the term "secular" and his description of the Center's dilemma as a choice between "secularism and sectarianism" (pp. xiv, 139). As his own account shows, the Bachemites at no time contemplated transforming the Center into a *secular* party but sought rather to make it into a nonsectarian "Christian" party that would continue to pursue clerical goals in cooperation with the supposedly equally "Christian" Conservative Party, the political defender of the Evangelical Church. Such a

partnership had been formed temporarily to promote the unsuccessful school bill of 1892, which attempted to secure and perpetuate Prussia's denominational school system against the wishes of the secular parties in the legislature. The Prussian Center allied with the Conservatives and "appeased" them by opposing suffrage reform in that state, as Ross says, but not merely to promote political and social parity and common economic interests; it was vitally important to maintain the alliance in order to prevent a threatened change in state educational and religious policies. The "Berlin orientation" was skeptical of the Conservatives' sincerity in defense of clerical goals and hoped to achieve them by extraparliamentary means, as Kopp had been able to do in the dismantling of the *Kulturkampf* legislation. Neither side in the controversy ever considered abandoning the clerical program.

ELLEN L. EVANS

Georgia State University

MARJORIE LAMBERTI. *Jewish Activism in Imperial Germany: The Struggle for Civil Equality*. (Yale Historical Publications. Miscellany, number 119.) New Haven: Yale University Press. 1978. Pp. xii, 235. \$17.50.

Deeply marked by the Nazi experience, much of German Jewish history has been colored by assumptions of Jewish timidity and wishful thinking and of unrelieved German ill will. It is the great strength of Marjorie Lamberti's study that it does not succumb to such nonhistorical reasoning. Hers is a history of the political activities of the Jewish self-defense organization, the *Centralverein*, during the two decades prior to World War I. Firmly lodged within the broad context of German political life, the study examines the organization's quest for support from the gentile community. Less directly, it addresses the larger issue of the Jews' search for their own political identity. Lamberti convincingly argues that the decision in favor of Jewish political activism was not only courageous and realistic but also partially productive.

Virtually from the moment of its founding in 1893, the *Centralverein* identified Germany's left-liberal Progressive Party as its most useful political ally. Especially from the time of the state and national elections of 1898, it provided the Progressives with financial aid while seeking to influence them to defend Jewish interests and nominate Jewish candidates for public office. But, as Lamberti points out in considerable detail, the cooperation of Jews and Progressives was seldom unstrained. The former urged their partners to throw their

weight against anti-Semitic candidates even if it meant endorsing Social Democrats, while the Progressives feared the Socialists as more deadly long-term enemies of liberalism. The leaders of Jewish self-defense, motivated by a renewed sense of religious pride, criticized the Progressives for nominating baptized Jewish candidates, a practice that the liberals could only defend on the basis of freedom of religion. The school issue, too, caused friction between the partners, with Jewish militants supporting Jewish schools (if somewhat equivocally) and Progressives backing interconfessional education. Lamberti congratulates the *Centralverein* for pragmatically supporting the Progressives even in the face of these frictions and of others caused between 1907 and 1909 by Progressive dallying with the bloc of parties close to Imperial Chancellor Bülow. In maintaining support for the Progressives, the *Centralverein* realistically rejected Zionist demands that the Jews form an independent political force on the grounds that a small minority alone could not hope to have any significant impact.

Lamberti accepts rather uncritically the Jewish leaders' identification of the Progressive Party as their only reliable ally in the fight against discrimination. In fact, they missed opportunities to enlist the Social Democrats and, to a lesser degree, the Catholic Center Party more directly in the struggle for Jewish equality. To be sure, she correctly notes that Socialist hostility to organized religion and Catholic sensitivity to liberal anticlericalism complicated these parties' relations with the Jews. She also points out that a new generation of young, militant *Centralverein* leaders after 1910 took a more positive view of the Social Democratic Party's stand against anti-Semitism. Continued friction between that party and the Jews must be viewed, however, against the background of Jewish preoccupation with civil equality at the expense of social reform.

More convincingly, Lamberti concludes that Jewish defense activities in a partly hostile environment instilled pride in German Jews and dramatized the larger issue of civil rights before the whole German nation. Her thoroughly researched monograph is a welcome supplement to the recent studies on related topics by Ismar Schorsch and Arnold Paucker and a valuable corrective to another by Jehuda Reinharz.

DONALD L. NEWYK
Southern Methodist University

PETER GAY. *Freud, Jews and Other Germans: Masters and Victims in Modernist Culture*. New York: Oxford University Press, 1978. Pp. xx, 289. \$12.95.

In *Freud, Jews and Other Germans* Peter Gay gives us a collection of some of his recent writings. The collection is an unabashedly personal one, by a historian who holds that in the right hands "private perspectives, . . . passion, even prejudice, may provide access to insights closed to cooler, more distant researchers" (p. ix). A Jewish refugee from Nazi Germany, Gay reaches for a new understanding of German history liberated from an exclusive, and therefore falsifying, vertical axis pivoting on the Nazi period. Gay's cautions about the dangers of retrospective determinism will come as no surprise to his fellow historians, but they are important concerns to be shared with the general reader.

As we have learned to expect, Gay appears here too as the revisionist, insisting on a closer examination of accepted dogmas. A clear partisan of Freudian psychoanalysis, Gay defends the master against the charge that his system was merely the time-bound product of turn-of-the-century Vienna. Rather, Freud's mental world was broadened by the perspectives of "the international positivist tradition, the tantalizing triumphs of classical archeologists," the constructions of Charcot, "the consolations of his far-flung correspondence," and the "infinitely instructive surprises of systematic introspection" (p. 34). Gay presents Freud—the analyst of the irrational—as a scientific rationalist who must be included in Modernism properly understood.

Gay's wide-ranging study of German Jews in Wilhelminian culture argues that they were not, as many enemies and some supporters have contended, the very embodiment of Modernism in Imperial Germany, either corrosive or creative. "Many Jews were in fact anything but avant-garde," while many "advocates of Modernism . . . were not Jews" (p. 158). Although German Jews did make "distinct contributions to German culture," they did so "as Germans far more than as Jews"; they "thought and acted like Germans" (pp. 93, 95). Here Gay's argument, though dazzling, is not entirely conclusive, for it is marred by the difficulty of resolving the historically and historiographically vexing question of what constitutes Jewishness, and by the inherent difficulty of demonstrating his proposition.

By way of drawing up a cultural tally, Gay evaluates Jewish tastes and the contributions of leading Jewish figures in painting, philosophy, literature, criticism, and journalism. He argues that in painting, a realm to which both Jewish and German traditions were equally inhospitable, Liebermann alone was distinguished but not a true rebel. In literature, Wassermann, who enjoyed an "inflated reputation," was not avant-garde. But Hermann Cohen and Elsa Lasker-Schüler, both Modernists, were surely animated by a Jewish

spirit, and Georg Simmel, Aby Warburg, and Carl Sternheim were so peculiarly individual and so devoid of any Jewish orientation as to make their examples appear irrelevant. For all its energy, Gay's argument is uncertain. ("That Jews were intimately involved in the making of a new culture had its elements of truth, but it was only half the truth, or less" [p. 161]). Gay concludes more correctly but less dramatically that German Jews were thoroughly embedded in their cultural context.

"The Berlin-Jewish Spirit: A Dogma in Search of Some Doubts" re-examines the existence of that spirit, an "ambivalent" idea at heart, difficult to delineate because of the indeterminacy of its elements but persistent and evident in the anti-authoritarian humor that survived in the Nazi period. Gay is at his best when being discursive and descriptive—as in his essay on Hermann Levi. It is as much a glance at the Wagner cult as a study of Wagner's Jewish conductor, whose relationship with the composer Gay explains through Jewish self-hatred. "Aimez-vous Brahms? On Polarities in Modernism" revises the accepted view of Brahms as a Romantic, painting him instead as a Modernist. It also recalls an earlier day when Brahms was regarded as difficult and inaccessible, rather than easy and popular. A final essay, on Eduard Hanslick, attempts to rehabilitate the reputation of this Viennese music critic—in Gay's view an important participant in the Modernist enterprise.

Gay is a connoisseur of style, and his book demonstrates the virtues and the dangers of history-as-literature. His admirers will delight in this new display of his skill: informed, confident, expansive; highlighted by the immediacy, the mannerisms, and the spontaneous self-corrections of speech; and presented as a personal statement where the frequent intrusion of the writer's voice seems natural. This book is always well written, though not always well argued, and Gay's lavish prose sometimes threatens to overwhelm its subject. His cleverly turned quip about the Warburg library, with its complex holdings being "the most Jewish of Creations" because the Jews are "the people of the book" (p. 130), suggests a peculiar application of the term "Jewish" and blurs a central concept in Gay's analysis. Indeed, Gay fails to produce an accurate definition of Jewishness. His pronouncement that "what preserves the Jew's separateness . . . is far less his ancient religion or some distinct culture than his terrible memories; it is Hitler who has defined the modern Jew and continues to define him from the grave" (p. 109) is partial to the point of error.

STEPHEN POPPEL
Bryn Mawr College

MAX SPINDLER, editor. *Bayerische Geschichte im 19. und 20. Jahrhundert: 1800–1970*. Volume 1, *Staat und Politik*; volume 2, *Innere Entwicklung, Land, Gesellschaft, Wirtschaft, Kirche, geistiges Leben*. Reprint. Sonderausgabe. Munich: Verlag C. H. Beck. 1978. Pp. xxxv, 644; xi, 648–1398. DM 74.

One special feature of German history is its strong regional diversification and its concomitant gamut of decentralizing or centrifugal tendencies, such as *Federalismus*, particularism, or separatism. The feature is singular in European history (with the possible exception of Italian history) and has created such methodological headaches for historians that they often tend to ignore it, resorting instead to the time-honored device of construing Prussian history as a kind of vicarious German history. For the growing number of historians dissatisfied with this situation, the work under review offers material help if not methodological relief.

The book covers the region of Bavaria and, chronologically, the nineteenth and twentieth centuries. It is designed as a *Handbuch*, a traditional form of German historiography emphasizing encyclopedic knowledge and reference over historical synthesis and interpretation. As such it is the fourth volume of a comprehensive work that covers all of Bavarian history. In general, this fourth volume is highly successful in its purpose; historians will find here solid help for whatever need they have for knowledge of Bavarian history. Indeed, in its main task, the full listing and concise discussion of the existing literature, the book is superb; just a look at the bibliographical references will stun readers. It should not discourage them, however, as the authors generally succeed very well in harnessing the bibliographical juggernaut.

Considering the encyclopedic purpose, it may seem acceptable, perhaps even advantageous, that the editor has separated political history (part 1) from social, economic, and intellectual history (part 2). It is also evident, though, that Bavarian social and economic history still have a less broad research basis than do political and intellectual history. Given the fact that the book is a collaborative effort (of no less than twenty-five authors) the degree of success with which the encyclopedic emphasis has been maintained naturally varies among the authors. Yet the quality of scholarship is generally high, and it would be misleading to single out, with so little space for differentiating discussion, individual authors for praise.

The *Handbuch* is less successful in dealing with Bavaria's ambivalent relationship to Germany and is, hence, less helpful to the historian in resolving the problem of German particularism. Perhaps inevitably, Bavarian history tends to appear here as a self-contained process with loving emphasis

on the strength of the autonomous Bavarian traditions and on the healthy liberalism and reform-mindedness prevailing in Bavaria's political climate, especially prior to 1871. Also inevitably, this will raise questions with regard to the apparent ease with which Bavarians abandoned independence in 1866-70 or with regard to the Nazi takeover in 1933, when an effective Bavarian resistance might even have rendered a great service to Germany.

It is true, of course, that a *Handbuch* cannot engage at length in interpretative or analytical discussions, and it is also true that discerning readers will find in these essays implicit answers to many if not all of their questions. But there are things that they will not find—a discussion, for example, of the fact that during much of the nineteenth century Munich was the successor to Weimar in its role as a cultural capital of Germany. And they may wonder why the chapter on the crucial, and most heavily researched, unification period had to suffer from an altogether insufficient space allotment. In short, one would have hoped that the authors had been more aware of the risk of confirming the misleading impression that Bavarian history was merely a backwater of German history and that what really mattered was indeed Prussian history.

WOLFGANG SAUER
University of California,
Berkeley

ULRICH LINSE, editor. *Gustav Landauer und die Revolutionszeit 1918/19: Die politischen Reden, Schriften, Erlasse und Briefe Landauers aus der November-Revolution 1918/19*. Berlin: Karin Kramer Verlag. 1974. Pp. 298.

This well-edited collection of Gustav Landauer's political speeches, letters, and writings is a welcome contribution to the growing literature on Landauer (1870-1919). It is also a valuable addition to the literature on German anarchism. The editor, Ulrich Linse, completed his doctoral studies a decade ago at the University of Munich under the guidance of Karl Bosl. Since that time Linse has produced a number of excellent books and articles on German anarchism and on Gustav Landauer, which, unfortunately, have received little attention in the United States. In this book Linse maintains the high standard of scholarship demonstrated in his previous work.

The collection consists of 117 items by Landauer or pertaining to him and the part he played in the revolutions that occurred in Munich during 1918-19. The majority of the selections, each with full

bibliographical citation, are by Landauer himself. The documents are grouped under the three separate revolutions that occurred in Munich: (1) November 7, 1918 to the murder of Kurt Eisner on February 21, 1919; (2) February 21, 1919 to the proclamation of the first *Räterepublik* on April 7, 1919; and (3) from the proclamation of the first *Räterepublik* to the end of the second *Räterepublik* on May 1, 1919. A chronology of events during the period precedes each section.

It should be emphasized that this is not the usual collection of documents. Of the 117 selections, 27 are likely to be found only in large libraries, and an additional 67 can be located only with great difficulty in special collections. The remaining 23 documents are archival sources taken from the following archives: Gustav Landauer-Nachlass, The Jewish National and University Library (Jerusalem); Landauer Archiv, Internationales Institut für Sozialgeschichte (Amsterdam); Deutsche Akademie der Künste zu Berlin (East); Staatsarchiv München; and Bayerisches Hauptstaatsarchiv München.

Some other important features of the book include photos of Landauer and some of his contemporaries; an index; a six-page listing of significant biographical events in Landauer's life; and a nineteen-page bibliography of books, articles, and archival materials both by Landauer and pertaining to his entire life and career. The bibliography would be extremely useful for someone planning a study of Landauer, because Linse sought to make it as complete as possible. There is an analytical introduction of 28 pages in which Linse demonstrates his grasp of recent scholarship on Landauer while attempting to probe the depths of this complex and multifarious thinker of the final years of the Wilhelmian Reich.

It is difficult to place Landauer in a single category or single school of thought. Various writers have found different things in his writing. Linse notes, "In einzigartiger Weise verbindet er deutsches romantisches Denken, jüdisches Erbe und die sozialistisch-anarchistische Tradition der Bismarckzeit und formte sie um in das, was er selbst einen 'Verwirklichungs-Sozialismus,' andere einen 'Kultursozialismus' nannten, ohne damit Landauers Verbindung von 'Mystik' und sozialer Utopie voll gerecht werden zu können. Und auch seine neuerliche Klassifizierung als 'romantischer Sozialist' übersieht . . ." (p. 9). Linse points out, and correctly, that to Landauer revolution was not an overthrow of the socioeconomic structure but a working toward socialism by mankind. Perhaps it is the irony of his participation in the brutal Munich revolution that makes him so interesting to present-day students. It is truly ironical that Landauer, who was essentially a pacifist,

was taken up and eventually consumed in the revolutions that raged in Munich.

Linse is to be commended for bringing together this collection of documents. He makes easily accessible, in a scholarly form, many items that hitherto could be found only in crumbling newspapers, little-known pamphlets, and diverse archives. This collection, in addition to bringing out the role of Landauer and anarchism in the Munich revolution, provides the reader with documentary material that demonstrates the reaction of the government and the Freikorps in Bavaria to the revolution. It has long been the opinion of the reviewer that a study of the part played by the anarchists in the ill-fated Munich *Räterepublik* would be profitable for understanding how the NSDAP was able to establish a foothold in Munich.

ANDREW R. CARLSON
Kalamazoo, Michigan

JAMES M. DIEHL. *Paramilitary Politics in Weimar Germany*. Bloomington: Indiana University Press. 1977. Pp. x, 406. \$17.50.

The novel role played by paramilitary organizations in the political life of Germany's first republic has long been recognized, yet this volume represents the first attempt to catalog and analyze in comprehensive fashion the nature, development, and role of those organizations. As such, it is a welcome addition to the literature on Weimar Germany.

James M. Diehl divides the history of Weimar's paramilitary organizations into three phases. During the first, which began shortly after the revolution, the new republican governments, frightened by the specter of a Bolshevik-like upheaval, encouraged the formation of a variety of illegally armed volunteer organizations: the well-known *Freikorps* as well as auxiliary volunteer units and *Einwohnerwehren*. This phase of government-sponsored paramilitary activity came to an abrupt end, according to Diehl, at the time of the Kapp Putsch, which revealed the readiness of many of these volunteer units to side with a rightist attempt to overthrow the republic. A second phase then ensued, during which a variety of illegal *Wehrverbände* attempted to perpetuate clandestinely the activities of the original volunteer units, retaining secret caches of weapons and engaging in plots to assassinate republican leaders. That phase in turn concluded with the debacle of the Munich Beerhall Putsch of November 1923, which discredited domestic guerrilla warfare. After a period of uncertainty and groping, there followed, Diehl finds, a final phase during which the *Wehrverbände* gradually abandoned the idea of an armed overthrow of

the government in the face of the apparent stabilization of the republic and the return of economic prosperity. Reluctantly, they began to augment their paramilitary exercises and their uniformed marches through Germany's streets with hesitant political activity within the hated framework of the republic, becoming in the process political *Kampfverbände*. In response to these developments on the Right, the Left and Left-Center responded by forming similar organizations of their own, setting the stage for the street brawls between forces of the Left and Right that came to be a distinctive feature of Weimar politics.

Diehl sees in this process of partial political domestication the seeds of disintegration for the rightist *Kampfverbände*. The decision to venture into republican politics immediately cost those organizations many of their die-hard antirepublican members. Exposure to the full range of controversial institutional, economic, and social issues with which the republic's politics abounded also set in motion centrifugal forces that were to weaken the unity and cohesion of most of the rightist *Kampfverbände*. Their decline redounded mainly to the advantage of the party armies, a form of paramilitary organization thoroughly subordinated to a specifically political organization and thus largely freed of the internal dissension over goals, strategies, and tactics in the political arena that undermined the cohesion of the political *Kampfverbände*. And of these party armies the chief beneficiary, according to Diehl, was the Nazi SA, which fell heir to much of the former clientele of the rightist *Kampfverbände* and far outstripped these in competition for the loyalties of youthful recruits during the late Weimar period. The paramilitary organizations thus not only injected a pernicious tradition of violence into Weimar politics but, by discrediting themselves through the failure of their ventures into the political sphere, also unwittingly bequeathed a potent reservoir of restless, violence-inclined manpower to Nazism.

Diehl presents his analysis on the whole in judicious and insightful fashion and writes a clear, forceful prose. The book is not, however, without a few shortcomings. Some well-known events, such as the Munich Beerhall Putsch, are recounted at excessive length, thereby temporarily obscuring the principal concerns of the book. Occasionally, the author indulges in a degree of hyperbole that casts doubts upon his critical judgment; he informs the reader, for example, that "every hue of the Weimar political spectrum was eventually represented by a paramilitary formation" (p. 21). There are still other unsupported assertions about the extent and pervasiveness of the paramilitary organizations; the reader learns that "millions of ordinary middle-class Germans had either participated in or otherwise supported antisocialist para-

military activity in some form or another long before the Nazis made their final, successful bid for power" (p. 22). Indeed, at times an otherwise uninformed reader might gain the impression that paramilitary politics occupied the center of the political stage throughout the republican period, whereas the parties led only shadowy existences by comparison. Finally, Diehl offers little more than a very general set of correlations and inferences in support of the contention that there was a major migration of paramilitary men from the rightist *Kampfbünde* to the SA during late Weimar.

For source materials Diehl has carefully plumbed the archives of West Germany, drawing with particular effectiveness upon the records of the republican agency in charge of monitoring potentially subversive organizations. But most of the information about developments within the various paramilitary organizations is derived from the author's skillful mining of numerous monographs and unpublished dissertations in which these developments have previously been examined, almost without exception, in isolation. The book is thus in large measure a perceptive work of synthesis and an instructive reminder of the value of that genre of historical analysis in an age when easy access to archives too easily gives rise to the assumption that original history must be written primarily from documentary sources.

H. A. TURNER
Yale University

BARBARA MILLER LANE and LEILA J. RUPP, translators. *Nazi Ideology before 1933: A Documentation*. Introduction by the translators. Austin: University of Texas Press, 1978. Pp. xxviii, 180. \$12.95.

This work, a useful anthology of early writings by the chief Nazi ideologists, brings together material that, for the most part, is either not easily accessible or not available in English translation. Represented here are Dietrich Eckart, Alfred Rosenberg, Gottfried Feder, Joseph Goebbels, Gregor and Otto Strasser, Heinrich Himmler, and R. Walther Darré. In addition, there are a number of party manifestoes and programs. Although much of Nazi thought is tedious, badly written, and often poorly rendered into English, Barbara Miller Lane and Leila J. Rupp have provided quite readable translations. There is an excellent introductory essay by Lane that originally appeared as an article in *Central European History* in 1974. The footnotes and biographical sketches of the principal Nazi writers provide a great deal of bibliographical and background material.

The readings trace the development of Nazi ideology, beginning with the Munich group of Feder, Eckart, and Rosenberg, who stressed respectively

breaking the bondage of interest and the corporative state, anti-Semitism, and the "Jewish-Bolshevik conspiracy." The second group of readings illustrates the Strasser faction, which developed such themes as the importance of the peasantry, the second revolution, and the military as training ground for citizenship. Interestingly enough, Hitler's name is rarely mentioned in these readings.

Among other things, Lane argues that Rosenberg may have introduced Hitler to Aryanism, not vice versa. She also contends that Gregor Strasser was the more original thinker and his brother Otto the more derivative. Otto's break with the party may have had more to do, furthermore, with his national bolshevist tendencies than with his rejection of the coalition government in Thuringia. Darré's importance may lie more in the formation of *Blut und Boden* ideology than in articulating the first Nazi agrarian programs.

Since most of the ideologists played little or no role after 1933 and since the earliest explanations of Nazism de-emphasized the ideological component, it is Lane's thesis that not enough attention has been paid to early Nazi ideology. The essential questions raised by this book, however, involve the impact of ideology on both Hitler's ideas and on post-1933 Nazi institutions. Lane does not attempt to answer such questions definitively. The relationship of these ideologists to Hitler's ideas must, she says, await a thorough examination of Hitler's unpublished speeches. While drawing parallels between such institutions as the Labor Front and Feder's corporative ideals, she does not pursue a detailed analysis of the relationship between early ideology and later institutions. Lane reminds us, however, that there is no "necessary relation between personal political power and implementation of ideology in the Third Reich." Those who rose to power after 1933 received their political education from the early ideologists. "They may well have attempted to put into practice the ideas of their mentors." It is to be hoped that we will soon have more detailed studies of the relationship of ideas and power in Nazi Germany in order to evaluate fully the historical significance of early Nazi ideology.

DONALD E. THOMAS, JR.
Virginia Military Institute

TATIANA METTERNICH. *Purgatory of Fools: A Memoir of the Aristocrats' War in Nazi Germany*. New York: Quadrangle, 1976. Pp. x, 285. \$15.00.

This book has the curiously ambiguous subtitle, "A Memoir of the Aristocrats' War in Nazi Germany." That might suggest that the book is a memoir of someone close to that small circle of German aristocrats who, in acts of defiance and

open resistance, displayed the civil courage they hoped would provide the moral capital for the reconstruction of Germany after Hitler's defeat. And indeed, Tatiana Metternich did, for a period, hold a minor post in the German Foreign Office that put her in proximity of such heroes of the resistance as Adam von Trott zu Solz. Nevertheless, despite their antipathy for the Nazis, neither she nor her husband, Paul Metternich, the great-grandson of the famous Metternich, were themselves involved in the resistance movement. Therefore, those who may turn to this volume for some new insight or information about the aristocratic resistance to Hitler will be disappointed. It offers nothing new.

Instead, it is a personal memoir, the story of two young aristocrats who met, fell in love, and married in the midst of the confusion and desolation of the Second World War. The fact that both were from the ranks of high nobility is incidental. Tatiana Metternich takes pains to show that the war was no respecter of persons—the Metternichs suffered like everyone else, except that they had more to lose: two large estates in the Sudetenland that fell to the Russians in 1945 and the estate on the Rhine, famous for its vineyard, *Schloss Johannisberg*, which was leveled by an aerial bombardment in 1945.

If the book adds little to our understanding of the "aristocrats' war," what if any, is its value to the historian? Actually, the fact that it was written at all is of some interest. These days, memoirs are usually written by "public" persons—statesmen, politicians, *literati*, and artists. "Private" persons do not ordinarily publish autobiographies. In writing her story, Tatiana Metternich reflects an older vision of society—one in which highborn nobles were, by definition, "public" persons whose lives were worth writing about. She writes with some nostalgia about the values stressed in her childhood in a high Russian noble family before the revolution. "Discipline was rigid, both at school and at home, to counterbalance privilege and luxury. There was a certain emphasis on ceremony, which was considered important to fix things in one's mind and breed respect." She takes seriously *noblesse oblige*; but so also does she take seriously the kind of deference that high station commands.

Tatiana Metternich is the daughter of a Russian nobleman with close ties to the imperial court. Forced into exile by the revolution, the family drifted from one part of Western Europe to another, supported for a time by the small income from its Lithuanian estates that had not been confiscated. For the historian, these segments of the book may be the most interesting for they offer a small glimpse of the world of a generation of aristocratic exiles. It is, as it were, an epilogue to

aristocracy. Although often financially poor, this aristocracy retained an important share of symbolic capital—well-known family names, connections in all the capitals of Europe, social graces and linguistic skills, and, above all, relatives scattered across the continent—which could open doors of opportunity, make "good marriages" possible (as to a Metternich), and enable the class to secure its survival.

As history, this memoir has little to recommend it; as a social document, it is of moderate interest.

ROBERT M. BERDAHL
University of Oregon

JOSEPH BORKIN. *The Crime and Punishment of I. G. Farben*. New York: Free Press. 1978. Pp. 250. \$10.95.

Whoever considers economic history a dull subject will find evidence to the contrary in this book. It reads like a suspense story, albeit not always a pretty one, dealing with proud accomplishments as well as with corporate greed and exploitation and ruthless disregard for lives.

Prior to World War I Germany was the leading producer in the world in organic dyestuffs, pharmaceuticals, and synthetic chemicals. In order to control competition, some of the largest German chemical companies formed a trust, which in 1925 became the *Interessengemeinschaft Farbenindustrie*, commonly known as I. G. Farben. These trust companies could look back upon an impressive list of achievements and Nobel prizes. From their test tubes had come Salvarsan, sulfa drugs, aspirin, novocaine, and ammonia, without which the country could not have continued the war effort beyond 1915.

Joseph Borkin focuses on I. G. Farben's role within the German economy: its partnership with Standard Oil of New Jersey, its part in World War II, and its trial at Nuremberg. The Farben partnership with Standard Oil committed both sides to an open exchange of information, but, under orders from the German government not to divulge any technical know-how, I. G. Farben officials succeeded in getting the better of their American counterparts. Driven by greed for profits and expecting a monopoly position in the United States, Standard Oil failed to comprehend the political situation, was naive in its trust of I. G. Farben negotiators, and actively discouraged other American companies from conducting research and development of synthetic rubber. In Germany the synthesis of both rubber and fuel was a vast success, enabling German tanks to roll and planes to fly until Allied bombers put an end to production in 1944.

With the benefit of hindsight, Borkin judges the conduct of Standard Oil too harshly. Granted, Standard Oil's officials were too intent on carrying on business as usual, but they were not truly criminals. The author is equally tendentious in his discussion of the phenomenal success of I. G. Farben's successor companies after 1945. He is indignant that Bayer, BASF, and Hoechst individually are now even more powerful than I. G. Farben. Borkin is on much safer ground when he condemns the astonishingly light sentences given to I. G. Farben officials at Nuremberg in 1948. For here was indeed the record of much crime and amazingly little punishment. Charged with exploitation and plunder of occupied territories and slavery and mass murder at I. G. Farben's Auschwitz plant, not a single convicted individual remained in prison after 1951. By that time, the Cold War was in full swing, and fear of communism was greater than revulsion at I. G. Farben's responsibility for atrocities.

Borkin's book is a fascinating account and indispensable reading for any student of European history, scholar and nonspecialist alike.

PETER W. BECKER
University of South Carolina

EGBERT KRISPYN. *Anti-Nazi Writers in Exile*. Athens: University of Georgia Press. 1978. Pp. xii, 200. \$12.00.

Egbert Krispyn, a professor of German at the University of Georgia, has written a broad overview of his topic that he hopes "may serve as a useful frame of reference" for more specific studies by others and admits his book claims "no original contribution to scholarship" except in the underlying "interpretive synthesis" (p. xii). While one can readily agree with the latter contention, although the synthesis seems fairly well hidden, one may doubt the usefulness of the book as intended. It is basically a string of stories about selected individuals among the exiled German writers.

Indeed, the brevity of the book is a serious problem not only because of the aspects neglected but also because of the matters Krispyn does cover. There is a serious imbalance in a short work on this subject which devotes so much space to "writers" such as Otto von Habsburg and Otto Strasser, contains yet another account of Nazi Germany's expansion between 1933 and 1939 (an entire chapter here), and presents a further chapter on those writers who remained in Germany during the fascist period. This is a major shortcoming.

The list of deficiencies is long; to mention a few of the more important will have to suffice. There is

no mention of the controversial, Brecht-Lukacs traditionalist-modernist polemic carried out in the exile press. Indeed, Krispyn does not seem to have used Brecht's *Arbeitsjournal*, which is surely a fundamental source on the subject of exiled German writers. Thomas Mann's *Tagebücher 1933-34* evidently appeared too late for consideration. Krispyn devotes far too little space to those writers who emigrated to the Soviet Union (only three paragraphs). A writer as important as Walter Benjamin is mentioned only once as a suicide, a tragic fate shared by far too many exiles. In terms of style there are some glaring vulgarities such as "he had seen nothing yet" (p. 7), Stalin as the "Russian strong man" (p. 55), "galloping alcoholism" (p. 78), and so on. These phrases are particularly noticeable in a volume devoted to writers and writing. Krispyn's historical judgment is also occasionally faulty as when he mentions FDR "bringing America into the war against strong isolationist currents" (p. 145). One wonders what effect Pearl Harbor or Hitler's declaration of war had on the United States entry into the war.

There are a number of positive points about Krispyn's book: it is good to see considerable space given to a neglected writer such as Gustav Regler, and Krispyn's evaluation of the value and effectiveness of exile writing is judicious as a whole while almost totally negative. Still, the deficiencies outweigh the positive aspects.

As an introduction to the subject for the uninitiated this book has serious shortcomings and offers little or nothing to the specialist.

BREWSTER S. CHAMBERLIN
Institut für Zeitgeschichte,
Munich

DANIEL J. NELSON. *Wartime Origins of the Berlin Dilemma*. University: University of Alabama Press. 1978. Pp. viii, 219. \$11.95.

The international political phenomenon of Berlin continues to hold the fascination of scholars of history and politics. The present volume, the first book-length study exclusively on inter-Allied wartime planning on Berlin, asks once more the question: How could it happen? How did the West allow a settlement in Berlin that was destined to lead to the later Berlin crises over access and the split of the city? In addition, the author seeks to derive insights about the problems of wartime diplomacy in our time.

In the main the book is an analysis and evaluation of the work of the European Advisory Commission (EAC), the London ambassadorial conference that hammered out the zonal agreements for Germany and Austria between January 1944 and

August 1945. Following closely the arguments of the U.S. EAC representative, Ambassador John G. Winant, and his advisors Philip E. Mosely and George F. Kennan, Daniel J. Nelson does not blame the EAC directly for the shortcomings of the Berlin agreements but rather the inflexibility of the Soviet delegate and, most importantly, the interdepartmental conflicts in the U.S. Government that left the U.S. negotiator often without instructions. On the basis of the best evidence available to the author—the State Department's EAC files, the Winant Papers, and Kennan and Mosely memoirs and interviews—he concludes that within the U.S. government it was primarily the obstinacy of the War Department's Civil Affairs Division and ultimately FDR's unwillingness to set down clear policy lines on postwar Germany that frustrated the work of the EAC. His final verdict on the EAC itself is nothing short of enthusiastic praise: "the EAC was certainly one of the most useful and most extraordinary bodies in the history of allied wartime diplomacy" (p. 170).

Unfortunately, our knowledge of the documentary evidence, especially that of the War Department, is too slim for such sweeping praise. Yet currently available evidence suggests that Nelson's overall assignment of blame is sound. The discomfort one feels in reading the author's assessment of U.S. policy is rather that it is completely out of touch with the significant revisions and contributions of the last ten years or so on U.S. foreign policy at the end of the war and on U.S. occupation. The last significant bibliographical entry dates from 1968, and the most recent book with which Nelson interacts is Jean Edward Smith's Cold War study, *The Defense of Berlin* (1963). Apparently, this is not entirely the author's fault, who did the last touch-ups of the book in 1972 (preface and brief commentary relating the work of the EAC to the 1971 Quadripartite Agreement). Rather, the book is an example of a most unfortunate delay in publication that will reduce its impact on the ongoing debate on U.S. policy and the Berlin problem.

DIETHELM PROWE
Carleton College

JÜRGEN BÜCKING. *Michael Gaismair, Reformer—Sozialrebell—Revolutionär: Seine Rolle im Tiroler "Bauernkrieg" (1525/32)*. (Spätmittelalter und Frühe Neuzeit: Tübinger Beiträge zur Geschichtsforschung, number 5.) Stuttgart: Klett-Cotta. 1978. Pp. 190. DM 54.

WALTER KLAASSEN. *Michael Gaismair: Revolutionary and Reformer*. (Studies in Medieval and Reformation Thought, number 23.) Leiden: E. J. Brill. 1978. Pp. x, 156.

Michael Gaismair, secretary to the Bishop of Brixen in the Tyrol, emerged into history in 1525–26 as one of the most outstanding and radical leaders in the so-called Peasant Wars, which included many town dwellers, among them Gaismair himself. His *Landesordnung* of 1526, despite some parallels with "utopian" documents from South Germany, really is unique. At first willing to compromise with his ecclesiastical and secular rulers, Gaismair became a dedicated revolutionary after witnessing Archduke Ferdinand's duplicity in dealing with the peasants and with himself (he had been elected as the peasants' leader in May 1525). Outlawed, he fled to Zurich and Venice, where he sought aid in destroying Habsburg power until his assassination in 1532. Damned by contemporaries, Gaismair has found sympathetic biographers only in the last century and a half. The fullest and best study until now has been Josef Macek's *Der Tiroler Bauernkrieg und Michael Gaismair* (1965), which provides the springboard for both the studies under review.

Jürgen Bücking of Tübingen University was already the author of three books and twenty-three articles before his untimely death in 1975 at the age of thirty-five. The editors of *Spätmittelalter und Frühe Neuzeit* have prepared this volume from a completed manuscript, with impressive results. Bücking's claim that he differs only partially from Macek, although true, is perhaps too modest, for Bücking meets Macek's Marxist interpretation on its own grounds by carefully analyzing the economic and social conditions in Brixen and Tyrol after 1500 in order to refute the *frühbürgerliche Revolution* theses concerning that region and Gaismair. This analysis consumes the first third of the book and tends at times to become an end in itself, since it is not always adequately tied to Gaismair's life.

The second, biographical chapter establishes Gaismair's career from 1525 to 1532 with great accuracy, occasionally correcting Macek. In particular, Bücking documents Gaismair's evolution from mediator and moderate in May–July 1525 to radical revolutionary after September 1525. In part the thesis of change rests upon the assertion (p. 63) that a long-known document of May 1525 really is a "first constitution" (*Landesordnung*) presented by Gaismair to the revolutionaries after they had elected him their leader. However interesting, the argument requires more support than it receives here. If true, it would add significantly to the tiny inventory—about ten to twelve pages—of Gaismair's extant writings.

Chapter three explains Gaismair's treatment by historians through four hundred years in terms of the writers' political affinities and sets the background for Bücking's own interpretation of Gaismair as a basically religious personality. The brief fourth chapter (eight pages) places Gaismair's

thought in a broader context and leans heavily upon Max Weber for its theoretical structure. Bücking contends that the revolutionary, eschatological program in the *Landesordnung* of 1526 rested upon Gaismair's belief that religion must redeem the lower layers of society and rescue them from the sins of the upper estates (p. 135).

A series of seven excursuses completes the book. These include carefully edited texts of what Bücking calls the "first" *Landesordnung* of 1525 and the better known one of 1526; wage and price structures in the Tyrol, 1500-60; and a useful table of events in 1525-26 relating to South Germany, the Tyrol, and Gaismair. The editors have provided an extensive bibliography of works cited, but no index.

Walter Klaassen's study is in some respects more superficial, but his analysis of Gaismair's religious sources and ideas surpasses that in Bücking. Klaassen sets himself more limited goals: to fill the lacuna of materials on Gaismair in English; to portray the reformer as a man of his time, not a modern secularist as Macek often does; and to correct Macek's facts as necessary. One might argue that Klaassen opposes Macek's Marxist view with an equally one-sided religious interpretation, but without Weber's famous disclaimer. At any rate, he presents the details of Gaismair's career in chapter one and then turns to examine closely his religious and social views. He agrees that Gaismair developed into a revolutionary after the initial revolt and argues even more strongly than Bücking that he cannot be labeled an Anabaptist. The great strength of this book springs from precise and competent comparisons of Gaismair with Luther, Zwingli, and the Anabaptists—comparisons that lead to the conclusion that Gaismair was a loner, like Sebastian Franck, Michael Servetus, and Thomas Müntzer, "all of whom developed religiousocial syntheses of their own and who therefore joined no group" (p. 122). The man remains elusive, yet significant.

In two appendixes, Klaassen provides excellent translations of the *Landesordnung* of 1526 (incomparably better than that in Schapiro's *Social Reform and the Reformation* [1970]) and three important letters; he also gives a sketchy historiographical survey.

In sum, both these studies respond to Macek's masterly treatment and both add to our knowledge of Gaismair through independent research in archives and published works. Each uses a historiographical angle different from that of Macek to achieve a new interpretation. They agree in seeing Gaismair as a man of his own time and in stressing his religious thought as the decisive element in his social activism. Bücking dredges deeper, using a more sophisticated theoretical structure, while Klaassen supplies an excellent critique of Gais-

mair's religious thought in relation to his own age and to the Old Testament. These books should, ideally, be read together, though either will stand alone. For those who read German, choose Bücking; but for those who do not, Klaassen is solid and useful.

PHILIP L. KINTNER
Grinnell College

WILLIAM A. JENKS. *Francis Joseph and the Italians, 1849-1859*. Charlottesville: University Press of Virginia. 1978. Pp. 206. \$12.95.

William A. Jenks has presented a tightly written account of the young Emperor Franz Joseph's policies toward Italy during the decade from the revolutions of 1848-49 to the war of 1859. The study is based on the published volumes of diplomatic correspondence between the Italian states of that period and Austria, France, and Great Britain. In addition, the author has consulted unpublished material in the archives of Vienna, Paris, Rome, and Turin and has also drawn upon a number of secondary sources.

Jenks begins his diplomatic history with comments on the Austrian presence in Italy from 1815 to the outbreak of the 1848 revolutions. Plunging into the complications of the revolutionary biennium, he next seeks to analyze the relationship between Prince Felix zu Schwarzenberg (the new Emperor Franz Joseph's chief minister in Vienna) and Massimo d'Azeglio (the new chief minister of King Victor Emmanuel II in Sardinia-Piedmont). Schwarzenberg's premature death in 1852 brought an end to the possibility of a more conciliatory Austrian policy. Thenceforth, the young emperor assumed a more forceful and uncompromising stance toward the Italian problem.

In a section dealing with Austrian policies in the Kingdom of Lombardy-Venetia and in Trieste after 1849, Jenks observes that the Mazzinian uprising in Milan on February 6, 1853, was no real threat; nevertheless, the embarrassed Field Marshal Josef Radetzky and the immature Franz Joseph reacted excessively to it, sequestering the property of Lombard émigrés. As a result, Austria came to be condemned as a bully and tyrant, not just by the Italians but also by the British and French. Meanwhile, the conditions of the Habsburg Empire's satellite states—Parma, Modena, Tuscany, the Papal States, and the Two Sicilies—added nothing positive to Austria's image during those years.

Jenks devotes a chapter to the growing enmity between Count Karl Ferdinand Buol-Schauenstein (Schwarzenberg's successor) and Count Camillo Benso di Cavour (D'Azeglio's successor) from 1852 through the Crimean War. The complications

of the latter conflict deprived Austria of reliable friends in the chancelleries of the great powers. The Congress of Paris of 1856 produced criticisms of the Habsburg Empire for its poor judgment in Lombardy-Venetia and for its apparent collusion with the Papal and Neapolitan governments.

Young Franz Joseph realized thereafter that he had to risk his life by making a progress through his Italian lands and forced his brother, the Archduke Maximilian, to take charge of civil affairs in Milan. Despite the Archduke's constructive ideas for more lenient government in Lombardy-Venetia, Maximilian obtained no support either from his Italian subjects or from his own superiors in Vienna. Franz Joseph sided with his obdurate minister, Buol, in completely misreading the diplomatic situation in Europe on the eve of the showdown with Cavour's Sardinia-Piedmont and Napoleon III's France. In that crisis, Austria received no support at all from Prussia, Tuscany, Modena, or Parma. The author's conclusion, therefore, is that Franz Joseph must bear the blame for the loss of Lombardy and the collapse of Austrian prestige in Italy in 1859-60.

Jenks's monograph provides a conveniently structured and apparently reliable summary of Austrian policies toward the Italian states in the decade 1849-59. It is regrettable, however, that the author did not make a greater effort to explain the broader context for some of the diplomatic moves of the period. He tends to assume too much prior knowledge of the Risorgimento on the part of his readers. Jenks also could have devoted more attention to his literary style, which is often choppy and archaic. Although Jenks's principal aim is to elucidate Austrian policies toward Italy, the reader cannot help but wish that he had also given more attention to the policies of Cavour and of certain other Italian statesmen in this decade. Inclusion of a map would also have been helpful. It is also not clear whether it is Jenks or Field Marshal Radetzky who is responsible for the mixed metaphor about a "sword of Demosthenes" hanging over the heads of the Austrians in Italy (p. 48).

CHARLES F. DELZELL
Vanderbilt University

MAX E. RIEDLSPERGER. *The Lingering Shadow of Nazism: The Austrian Independent Party Movement since 1945*. (East European Monographs, number 42.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. xi, 214. \$14.00.

The publication of a book in English on the rise of the *Verband der Unabhaengigen* in 1949, its dissolution, and its transformation from a largely

anti-establishment party in 1955 to a more homogeneous nationalist party, the *Freiheitliche Partei Oesterreichs*, fills a real need. Max E. Riedlsperger successfully summarizes the background of this "third force" in Austria and then shows with considerable skill the two main convergent reasons for the formation of the VdU, namely, to provide a haven for politically orphaned ex-Nazis and to supply an organized protest against the disadvantages of the Socialist-Conservative monopoly and "stranglehold" on Austrian democracy after 1945. The author explains the link between the de-Nazification process and the early popularity of the VdU and clarifies the role that this fourth party (after the SPOe, the OeVP, and the KPOe) hoped to play, as well as the role it was actually forced to play, in parliament. The author shows the internal contradictions between "liberal" and "national" groups within the party, contradictions that contributed to its demise in favor of a more outspoken nationalist successor, which, however, had to "buy" a degree of homogeneity at the steep price of political isolation and the stigma of neo-Nazism, and which has to eke out a political living in the present decade of continued prosperity, absence of the "Red-Black *Proporz*," and a vanishing memory of political exclusion for ex-Nazis.

These phenomena are admittedly difficult to describe to an audience not acquainted with the details of Austrian political developments since World War II. Readers who are familiar with the "trust-busting" efforts of the VdU and the FPÖe, on the other hand, will find little in the book that is new to them. Riedlsperger, on the whole, writes an objective history of the Independent Party Movement, using personal interviews, newspaper articles, parliamentary speeches, and secondary sources. His voting tables are very helpful.

What is somewhat disturbing is the poor proof-reading job done on the manuscript. There are a number of typographical mistakes and misspellings ("forceably," p. 17; "Piefka," p. 28; "direly," p. 76; "Kirshschlaeger," p. 167; and a number of others). These uncorrected errors in orthography may be overlooked, one and all; but when one reads literally dozens of times the name of Leopold Figl as "Figil," one wonders whether the author bothered to read his own manuscript with much care.

A more serious matter than mechanical errors is the statement that Karl Renner emerged "from the ruins of a second lost war" (p. 17). Lost war? It would seem that World War II was "lost" only in the eyes of those who sympathized with the Nazis; in fact, the war was won for all Austrian patriots. It is hoped that this is not a Freudian slip on the part of the author. Moreover, misleading sentences like "the possibility in 1949 (*sic!*) of forging these

(diverse) elements into one political party was destroyed by the reduction of the severity of the National Socialist Law, by the end of occupation and above all by increasing prosperity" (p. 61), or anomalies like *Altmark* for *Altreich* (p. 28) can only detract from the respect the reader desires to muster for this otherwise important and interesting book.

ROBERT SCHWARZ
Florida Atlantic University

ERICH GRUNER. *Die Parteien in der Schweiz*. (Helvetia Politica: Schriften des Forschungszentrums für schweizerische Politik an der Universität Bern. Series B, number 4.) 2d. ed. rev. Bern: Francke Verlag. 1977. Pp. 351. 34.00FR.

Erich Gruner's book is a detailed, analytical study of the political parties in Switzerland. The historical part makes up not quite half of the text. The other half is given to general remarks about problems of methodology and approaches to the scientific study of parties in general, to the description of the internal life of the parties in Switzerland, and to an account of the changes in their function and role since the publication of the first edition in 1969.

Gruner is eminently qualified for his task. As founder and director of the research center for Swiss politics at the University of Bern he has done yeoman service through the years in building up the documentation needed for the scientific study of the Swiss parties in particular and contemporary politics in Switzerland in general. He is widely recognized an authority in this field, a fact that is reflected in his observation that the parties opened their archives to him and made materials available that they had kept secret as recently as a decade ago when he was working on the first edition of this book.

Gruner's is a pioneer study in the breadth with which it covers the spectrum of political party life in Switzerland. He points out the difficulties of such an undertaking, among them the fact that most national parties are not centrally administered entities but umbrella organizations for cantonal parties that have their own histories, organizational peculiarities, and successes and failures.

In most cantons the competing parties are close enough to each other to cooperate in governments made up of elected representatives from among them. This is also the case at the federal level. The highest executive of the land, the Swiss Federal Council (Bundesrat), is made up according to a "magic formula" of two Liberals, two Christian Democrats, one member of the People's Party, and two Social Democrats. All four parties represented

thus are governmental parties; but at the same time each retains to some degree an opposition character.

This was not always the case. For decades the *Freisinn* in its liberal, radical, and democratic colorings dominated Swiss politics. It made room only slowly to others, first to the Catholic Conservatives, then in 1929 to the Peasants Party, and, finally in 1943 and 1959, to the Social Democrats.

Gruner describes the historical development of each of these as well as of the other, smaller parties that do not form part of the Federal Council. Whenever possible he abbreviates the enormous variety and multiplicity of facts into generalizations. He finds, among many other interesting things, that citizen participation in party affairs is less democratic than in matters of local, cantonal, or federal concern, since members have few means to influence party decisions. He points out the dangers created by the death of good party newspapers.

Since the late 1960s party activities in Switzerland have become livelier again than they had been for several decades. New parties have come into existence, and old class and economic parties have remodeled themselves into people's parties. Efforts are made to enlarge memberships and give members a chance for greater participation.

Gruner's sympathies are clearly with the political parties rather than with their influential competitors, the lobbying groups of numerous special interest organizations. He thinks that parties should be anchored in the federal constitution and should be financially supported by the federal government. This would provide a chance to bridge the growing gap between citizens and state and would give the parties the weight due to their responsibilities and function as tools for the political well-being of the country.

HEINZ K. MEIER
Old Dominion University

GIUSEPPE DE CESARE. *La formazione dello Stato unitario (1860-1871)*. (Valori politici, number 21.) Milan: Giuffrè. 1978. Pp. 367. L. 9,000.

"Piedmontization," Italy's administrative unification process of the 1860s, is what this book is about. Despite the dates in the title, however, the coverage goes only to 1865, thus stopping before the acquisition of Venetia and Rome and their incorporation into the national fabric. What is lacking in the promised span of time is more than made up for in detail. Even so, this book is not a finished product.

Beginning with the fundamental issue of regionalism, debated and first defeated even before Ca-

vour's death, Giuseppe de Cesare takes up in exhaustive completeness the tale of every administrative proposal made in the Italian parliament until 1865 and the provincial and communal law of that year. From massive research he gives copious information on the debates about extending throughout the peninsula the judicial system, schools, taxes, and prefects of the Piedmontese government. Almost an endless procession of deputies and senators is quoted in every moment and nuance. Finally, with the debate over the transfer of the capital from Turin to Florence, one fears it may become a brick-by-brick process. Had the story been carried through to making Rome the capital, there might have been no readers for it in this version.

Reporting every comment and caveat does reveal something, although one wonders if such was De Cesare's intention. He does nothing to mask the administrative delays created by the Italian ruling class through its crass, smug, and pompous maneuvering. In the judgment of that group, there had been no Italian social revolution, and they had no desire that one should occur through any quirk that might creep into the new administrative structure. Their suave and sophisticated professions of liberalism did little to hide their basic fear of liberty and, most of all, their cynical suspicions of each other. Centralized government was little more than a guaranteed way of keeping close watch on each other, no matter the high-flown ideological justification given for it. If these men had shown the capacity for political wisdom that they professed, they might not have botched the final product. The result was a warping and wounding of the lives of citizens of the new nation. How much of it was the fault of Piedmontization and how much it was simply "malformation of the unitary state" by its founders is not clear. And that is where this book fails worst, even though it implicitly accepts the importance of men as much as issues.

Despite these shortcomings, scholars of European administrative history and development cannot ignore a work such as this, even if its structure makes it almost unreadable. The subject is still quite timely, as is recalled by the bitter battle of less than a decade ago when administrative regions were at last instituted in Italy. Twenty years overdue by the terms of the republican constitution, they nevertheless came over the emotionally alarmist opposition of the "enlightened" heirs of this earlier generation. It is a pity, then, that the background of the story is not clearly and completely told here.

Without chapters, the only divisions in this book are three so-called parts, each uneven and without so much as a paragraph heading, on one occasion

for 180 pages. There is no index of names, no bibliography, no introduction. Only a perfunctory, anticlimactic little conclusion gives any hint of the author's interpretations. What happened to produce this is impossible to say. Both this press and this author have previously distinguished themselves with excellent works in this field. To give such a topic the cavalier treatment found in this instance is unforgivable. Still the volume is worth some serious digging, but those who undertake the labor are urged to see first De Cesare's masterful *L'ordinamento comunale e provinciale* and Roberto Ruffilli's *La questione regionale*, both covering the years 1862-1942.

BENJAMIN F. BROWN
University of Kansas

PETER F. SUGAR. *Southeastern Europe under Ottoman Rule, 1354-1804*. (A History of East Central Europe, volume 5.) Seattle: University of Washington Press. 1977. Pp. xviii, 365. \$16.95.

This book is the only one, in a series of eleven volumes covering the history of East Central Europe, dedicated to the millenary Turkish and Ottoman presence in the area. The twelve chapters of the book, excluding conclusions, are divided into five parts that reflect the author's approach and handling of this exceptionally complex topic. The first part deals with the basic history and organization of the Ottoman state; the second is dedicated to the study of urban and rural life and taxation in the Balkans, that is, in the "core" areas of the Ottoman domains in Europe; the third covers the history of the tributary states; the fourth strives to analyze the socioeconomic factors causing the disintegration of Ottoman-Balkan society; and the fifth is a short essay on cultural life related only partially to the main theme. The conclusion, which is in fact a new chapter somewhat different in approach and emphasis from the rest, is followed by a lengthy and, at times, laudatory bibliographical essay, useful tables of sultans, vezirs, emperors, kings, princes, and so forth, and several glossaries.

In its conceptual design this is a pioneering study. Different from many other works on East Europe, especially on the Balkans, it takes into account the lasting impact of the economic, social, and political forces generated by the long Ottoman rule in the area and strives to make some use of the vital Turkish sources long ignored by most Balkanists. Moreover, Peter F. Sugar gives excellent treatment, within the same conceptual framework, to the history of Transylvania, the social transformation of the Hungarian plains, and the Porte's relations with the Habsburgs and the Poles.

Nevertheless, the inherent methodological and conceptual difficulties involved in the study of a complex medieval Muslim state such as the Ottoman Empire, which ruled diverse religious, ethnic, and social groups in an area stretching from the Adriatic to the Indian Ocean, unavoidably are reflected in Sugar's work. The fact that the subject is treated both chronologically and topically in chapters written apparently at great intervals of time reflecting the author's new knowledge and insight into the problem undermines the sequence and the cohesion of analysis. The use of sources, mostly written, is rather uneven.

Sugar makes a decided effort to look at the Ottoman system as a whole and to use Turkish sources, but he ends not only ignoring the work of many Turkish scholars as well as publications dealing with East Europe (such as *Güney Doğu Avrupa Araştırmaları Dergisi* [1971/2]) but also relying mostly on the work of a few Balkan scholars, some of whom are unduly overrated. Consequently, although Sugar is critical of the Balkanists' old ethnocentric view of history and of their implicit use of the conflict model, that is, of the idea of a continuous struggle between a "national-indigenous-Christian" group and an "alien-exploiter-Muslim (Turkish)" group as the dynamics of the entire sociopolitical system, he somehow manages to incorporate it in his analysis. Moreover, the tendency of Balkan historians to equate the local variations of Ottoman institutions and administrative practices with the system as a whole and to regard local events as representing universal trends seem to be accepted at face value. Thus, the Christian *hajducs*, whose sporadic existence was confined to mountainous areas and who had a counterpart among Muslims, become the symbols of the Balkan national resistance; the *yamaks*, the apprentice Janissaries, are transformed into petty merchants (p. 242); the *celeps* or cattle dealers found chiefly in Bulgaria become merchants active in the entire realm; and so forth. Incidentally, the Ottoman administration took a census every thirty, not every ten, years and that until about the end of the sixteenth century.

I also have some reservations about some of Sugar's views concerning the nature of the Ottoman system as a whole. He believes, for instance, that the key feature of the Ottoman state was its assumption of divine protection, a rather common claim among medieval states. Actually, the Ottoman state came to regard *adalet* (justice), a concept derived from a variety of medieval Muslim sources and practical necessities, as its fundamental principle of government and tried to shape its socioeconomic system, political organization, and philosophy accordingly. Again different from Sugar, who regards the *akhs* (mystical fraternities of guilds-

men) as the most important social group in the early Ottoman state, I would place the onus on the *uç-beys*, the lords of the marches. These latter became *de facto* rulers in the Balkans, challenged the authority of the sultan's central government, and precipitated early in the fifteenth century a social and political crisis that profoundly transformed the system as a whole. The *derwishes*, popular preachers who had a more vital role in the colonization of the Balkans than in creating an eclectic folk-Islam palatable to non-Muslims, as claimed by Sugar, acted with the permission and under the protection of the *uç-beys*.

As far as the downfall of the Ottoman system is concerned, Sugar, following the views of some Balkan scholars unfamiliar with the intricacies of the Ottoman land system, believes that the transformation of the *miri* (state) lands into *cifliks* or private estates caused the Ottoman state's disintegration in the seventeenth and eighteenth centuries. Actually since the title, *rakkaba*, of such lands was registered in perpetuity as a state property, the government could claim them back whenever it mustered enough power to do so. It is true that during the second half of the eighteenth century much of this land passed into the hands of the notables or *ayans*, but early in the nineteenth century the government not only liquidated the leading *ayans* but also confiscated much of their holdings and eventually introduced a new land code in 1858 that reasserted the government title to *miri* lands and created a new, far-reaching relationship between the land and its cultivators. I would say that the weakness of the Ottoman system resulted from its inability to reconcile its medieval, elitist political and social philosophy and organization and its statist concepts of property with the multi-sided, functional, interest-oriented demands of a nascent capitalist order stemming from the European economic and political domination of the Ottoman state. This produced a new pattern of socioeconomic organization and a series of conflicts based on religious and ethnic alignments. Eventually in the nineteenth century the Christians, benefiting from certain economic advantages and backed by Europe, became the representatives, as well as the beneficiaries, of a primitive, colonial type of capitalism developed in the Ottoman state. In studying the status of the non-Muslims Sugar relies on the late H. A. R. Gibb's view that they were treated as *zimmi* (*Dhimmi*), as was the case in the classical Arab states. Actually, in the Ottoman state social status, as different from legal status, was determined not so much by religion as by an individual's association with the government, which in turn determined the subject's tax status, income, social position, and prestige. Consequently, the *knez* and other *primküran*

(primates), that is Christian communal leaders, were exempt from taxes because of services rendered to the state in the administration of the local community and in the collection of tithes and taxes; therefore, they had a social position superior to the Muslim *raya* or peasantry. I believe that in studying the everyday cultural and social life of the non-Muslim population, Sugar should have placed additional emphasis on their basic organizational units: the *millet* at the national level and the communal organization at the local level.

There are many other points in Sugar's treatment of Southeastern Europe during the Ottoman era, including the profound cultural impact of the latter, which, in my estimation, need further elaboration, correction, and balance. Yet all these, and much of what I have written here, are matters of emphasis, interpretation, and opinion. In essence, this is a useful, thought-provoking book that I recommend highly. It is, in fact, an excellent foundation for developing new approaches and for giving a new impetus and direction to the study of Southeastern Europe.

KEMAL H. KARPAT
*University of Wisconsin,
Madison*

GERASIMOS AUGUSTINOS. *Consciousness and History: Nationalist Critics of Greek Society, 1897-1914*. (East European Monographs, number 32.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1977. Pp. v, 182. \$13.50.

Greece's defeat in the Greco-Turkish War of 1897 ushered in a decade of intense pessimism, political instability, and socioeconomic tension that precipitated a conscious critique and re-evaluation of Greece's culture as perceived through its dominant ideology, the *Megali Idea*. Broadly conceived, the *Megali Idea* represented a syncretization of Greece's classical and Byzantine traditions with the Greek people's sense of cultural superiority over their neighbors. It was a formulation of a historical consciousness and a national identity based upon a historical continuum tangibly expressed in the belief in Hellenism's mission to regenerate the East—a mission that entailed a policy of irredentist expansion in territories still under Ottoman rule. The *Megali Idea* came to be looked upon as a panacea for the nation's socioeconomic and political ills, and its failure at the turn of the century proved to be traumatic for an intelligentsia nurtured in such an ideological context.

In this study Gerasimos Augustinos examines, through the works of Kostas Palamas, Perikles Giannopoulos, and Ion Dragoumis, the spiritual crisis, the psychological strain, and the intellectual

incoherence that followed the defeat of 1897. Their conscious critique of Greek society was motivated by and aimed at a definition of their nation's cultural identity through a re-examination of Greece's historical past as a source of their nation's self-image. In an effort to depart from the traditional approaches to nationalism that emphasize either its ideological aspects or its actualization as a movement, the author attempts to stress an often neglected but "a persistent concern, that of national identity, and those who seek to define and shape it" (p. 2). Although Augustinos has found attractive Anthony D. Smith's analysis of ethnic nationalism, he has not taken full advantage of Smith's typologies, which associate the emergence of nationalism with the pressures for modernization. Not that the author does not stress adequately the crisis that usually follows an encounter between traditional cultures and the secular world of the West; rather, the problem lies in his exclusive dependence upon the intellectual sources of nationalism. Hence, he fails to transcend the realm of ideology as a precondition to a multidimensional analysis of the internal contradictions immanent in the relationship between modernization and resistance to Western cultural and economic penetration. It seems that Greek nationalism underwent a qualitative transformation at the turn of the century that was motivated not only by the question of national identity, but, above all, by the failure of the Greek state and Greek society to withstand successfully and to respond creatively to the pressures of modernization. Thus, while the leading nationalist critics of Greek society were philosophically nurtured within the framework of the West's contemporary neo-romantic, antirationalist currents, they sought their salvation in archaic cultural forms of their nation's past.

These observations are not meant to detract from the value of this book, which is an important contribution to our understanding of the intellectual climate of this period.

GEORGE B. LEON
Memphis State University

I. I. LESHCHILOVSKAIA. *Obshchestvenno-politicheskaia bor'ba v Khorvatii, 1848-1849* [Social and Political Struggle in Croatia, 1848-49]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 303. 1 r. 40 k.

Yugoslav historians have been diligent enough in illuminating the 1848 revolution in Croatia. Their diligence, however, has not been without a flaw: they have expended their labors in exploring particular aspects of the revolution, but they have been remiss in taking in the whole sweep of the

movement. The last Yugoslav historian to attempt to do so was Rudolf Horvat, in his narratively oriented *Hrvatski pokret god. 1848* (4 vols., 1898–99). Since then only Vaso Bogdanov has devoted a major monograph to the subject, but his *Društvene i političke borbe u Hrvatskoj 1848/49* is a compendium of the contemporary press rather than an analysis. So it has been left to I. I. Leshchilovskaia, an “outsider,” to come up with an up-to-date, critical history of the 1848–49 revolution. The work itself may be viewed as a sequel to her *Illirizam* (1968), which tackled the story of Croatia’s pre-1848 national revival.

Leshchilovskaia knows her Croatian history, and, to those able to overcome the language barrier, her account will flesh out the meager body of the West’s knowledge about Croatia. She takes the reader beyond the much-traveled ground, such as the Croatian-Magyar War of 1848 and the movement for Croatian-Serbian unity, and addresses herself to questions that have more universal applicability. She reviews the three principal ideological currents the revolution brought forward, conservative, liberal, and “bourgeois-democratic,” follows the social aspirations of the peasantry, and devotes a chapter to the work of the Croatian diet. She demonstrates the existence of sympathies between Croats and Serbs, a familiar subject, but also supplies evidence, not so familiar, testifying to sentiments of distrust (p. 184). Her book will induce the reader to compare the worlds of Zagreb and, say, of Paris. In the French parliament in 1848, it was (among other issues) the working class and universal manhood suffrage that agitated minds. In the Croatian diet, the latter did not merit even a whisper and the former was remote; the spotlight was on the peasant and on nationalism. The comparison adds up to a tale of two cities, both coming alive at the same time but moved by quite different concerns. Leshchilovskaia herself does not venture into such comparisons but furnishes ample raw material for them. She will surely get high overall marks for her effort. The trouble arises when she works up to her main conclusions: here her Marxist proclivities get in the way of careful judgment. As she would have it, the principal moving force in 1848 Croatia was the peasantry, but the “bourgeois” leaders failed to exploit its revolutionary potential, thus stalling the revolution half-way. The leaders showed insufficient devotion to revolutionary solutions; in particular, they were unwilling to resort to the liquidation of the Habsburg Monarchy of which Croatia formed a part. In effect, she taxes them with a failure to become republican firebrands. This is a bit like rebuking Aristotle for not being a good liberal democrat. What should the Croatian leaders have done? Unseated an emperor and pro-

claimed an independent Croatian republic? Would the peasants have understood, let alone supported such a solution? The peasants were interested in the abolition of serfdom and were quite willing to take matters into their own hands to accomplish this. But did this make them potential recruits for a republican cause? Not on the evidence presented in her book. And not, either, if we extrapolate from the attitude of peasants in other parts of Europe. Everywhere in 1848, even in turbulent France, the peasant tended to be politically conservative; to assume that the Croatian peasant would have behaved differently, if only his leaders had shown him the way, creates a credibility gap that cannot be closed by any available historical evidence. Fortunately, Leshchilovskaia’s volume does not stand or fall with her conclusion, and every reader will find enough substance and good judgment in it to make the reading worthwhile.

STANLEY Z. PECH
University of British Columbia

DUŠAN NEDELJKOVIĆ, editor. *Naučni skup: Svetozar Marković, život i delo, 24–27. novembar 1975*. [Symposium: Svetozar Marković, His Life and Work, 24–27 November 1975]. (Naučni skupovi, number 5; Odeljenje Društvenih Nauka, number 3.) Belgrade: Srpska Akademija Nauka i Umetnosti. 1977. Pp. 717.

In November of 1975 the Serbian Academy of Sciences organized a symposium to commemorate the centennial of Svetozar Marković’s death. The first important socialist in the Balkans, Marković had a varied and exciting career before tuberculosis ended his life short of his thirtieth birthday. Political organizer, journalist, social theorist, and state prisoner, Marković astonished and confused sleepy Serbia, which had seen no such comet in two generations and precious few in all its history. He was not an original thinker and he was a disaster as a politician. But he was a pioneer, however quixotic, and his courage inspires respect.

We must concede humane motives on the part of the Academy, but the published fruit of the 1975 symposium contains far more starch than spice. There are no fewer than fifty-five individual contributions in this volume and the quality varies wildly, though staying for the most part in the lower registers. Every conceivable bit of Markovićiana is examined under—in all save two papers—an ancient Marxist-Leninist microscope that no contributor is able to focus and hold steady for more than a fleeting moment. The sheer weight of this mass tends to trivialize the subject.

Dušan Nedeljković, who presided at the symposium, set the tone by referring to Marković in

terms that would lead the unwary to believe they were hearing the saga of Friedrich Engels. Another participant enthusiastically called Marković one of the "great men of science" and praised his "Marxist critical sociology." V. G. Karasev of Moscow contributed his usual commentary on Marković and the Russians; his material is still interesting but it lost its freshness fifteen years ago. Henryk Pisarek of Wrocław, also as usual at these affairs, offered a provocative reading of some neglected Marković texts. The late Jacques D'Hondt of Ghent offered an interesting paper on Benoît Malon's propagation of Marković's fame in France (Malon did the same for several Russian revolutionaries). The Yugoslav contributions to the volume, naturally the majority, strive to attain the mediocrity established by Nedeljković. They succeed.

The participants in the symposium agreed that Svetozar Marković was a historical figure of the first class, and most of them, excepting Karasev, and D'Hondt and Molnár who were not required to express opinions, referred to him as a Marxist. In the Serbian context they are right on the first count; by any standard they are wide of the mark on the second.

WOODFORD MCCLELLAN
University of Virginia

FRANK L. KAPLAN. *Winter into Spring: The Czechoslovak Press and the Reform Movement, 1963-1968*. (East European Monographs, number 29.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1977. Pp. vi, 208. \$14.00.

Most students of the Czechoslovak attempt to reconstruct communism view the role of the press as extremely important, and some have argued that it was crucial. Frank L. Kaplan rightly sees its dual nature: "It was the irony of the Czechoslovak experience in 1968, . . . that the very condition which was at the heart of the democratization process—an independent and vigorously active press—also represented a major factor in its demise" (p. 133). One could extend the author's reasoning even further: although freedom of expression may have been at the "heart" of the Prague Spring, a more coolly calculating "mind" would have held its avid promoters at bay while reforming political institutions and processes in a way that would not have made the Kremlin see red. The end result might of course have been more "Hungarian," and Czechoslovakia would not have had its historic moment of truth.

Be that as it may, the germination of defiance and reform in the official, especially cultural, press

has been a feature of Communist governance throughout the orbit to the point where it cannot be ascribed to the peculiar predilection of any one nation. (Next to it there has been the unofficial, underground press, be its form an antistate leaflet, a samizdat statement, a "parallel" newspaper, or a wall poster.) A historian of communism must pay attention to the phenomenon, and a cross-national comparative study would be very useful. I suspect that one of its conclusions would be that which Kaplan identifies at the beginning of his study: the character who seeks liberal change is not so much the archetypal newsmen, but rather the crossbreed between an author and a journalist, known in Eastern Europe as the publicist. From my own experience I would even venture formulating a thesis: being a Communist journalist pure and simple can turn a person jointly or severally into a swine, a hack, a faceless practitioner, a cynic, and a drunkard, but being a cultural journalist or a publicist can, though not necessarily, lead to loftier mental states. It is the injection of culture that elevates the human spirit, not the practicing of the journalist's profession. True or false? (I know, of course, that any of the miserable attributes can well relate to cultural journalists as well. Especially the measure of inebriation.)

Kaplan's study is a welcome contribution to the history of Czechoslovak reformism in the 1960s, above all because of its concise and clear contours. The narrative line is straight, there is virtually no padding, and the style is purposeful. One would, however, expect a little more from a specialized monograph, both in the sense of greater detail of investigation and in relating the press's story to the other factors of reformist change. Thus there is little or nothing about the provincial and specialized press (other than that of the Writers' Union), about branches of the Journalists' Union outside Prague, about the Press Department in the Dubček Central Committee, about the part played by journalists in the Coordinating Committee of the Creative Unions, about readers' letters, about investigative journalism, about the *Kultúry život* crisis in March-April 1968 (and the Slovak situation in general), about *Kultúrní noviny* and the planned *Lidové noviny* and *Občanské noviny*, to name but a few lacunae. By concentrating on writers and journalists, the author loses sight somewhat of the economy, the social scientists, the intraparty debates and battles, federalization, and other issues, all of them connected with the new role of the press in one way or another. In brief, the story is here correctly told in general outline, but one would gladly tolerate another fifty or even a hundred pages for the sake of greater empirical richness.

A cavil to end with: could not the Czech and Slovak diacritical marks be inserted and the spell-

ing errors avoided? And Milan Hübl (p. 48) is not a poet.

VLADIMIR V. KUSIN
University of Glasgow

BENJAMIN P. MURDZEK. *Emigration in Polish Social-Political Thought, 1870-1914*. (East European Monographs, number 33.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1977. xvi, 396. \$18.00.

Students of nineteenth-century Polish history are rarely treated to stimulating works of substance in the English language. Benjamin P. Murdzek's fine study of an exodus, the origins of which were infinitely complex, is primarily based on a diligent combing of the prodigious contemporary literature spawned by polemics on the relative virtues and vices of emigration from partitioned Poland during the forty-four years preceding World War I. Two hundred and eight pages of plump notes, bibliography, and population statistics might seem a little excessive to buttress a slim text of one hundred and eighty pages.

The author offers considerably more than the title promises, and the comprehensive scope of his work is truly impressive. Movements of population, whether seasonal or permanent migration within Continental limits or emigration overseas, have been deftly traced and judiciously analyzed against the background of the disparate and changing conditions in Poznań, the Congress Kingdom, and autonomous Galicia; the domestic policies and legislation directly or indirectly impinging upon the Polish subjects of the three empires; the practical, if largely ineffective, measures of self-defense initiated by politically conscious Poles; and, finally, in response to the persistence of rural emigration and the challenge of left-wing ideas during the 1890s, the crystallization of several schools of Polish thought. Controversy raged around such academic debating points as the value of exporting a surplus population whose remittances and savings were often invested in Polish land or nebulous dreams of Polish colonies in Latin America capable of exploitation in "the national interest." Neither outright hostility to, nor qualified support for, emigration could eliminate or regulate the peasant's consuming desire to improve his lot elsewhere.

Although Murdzek's meticulous documentation suggests a dearth of modern research on the subject, there have been forays into odd corners of the field by Polish historians and sociologists within the last decade. Articles containing relevant information are scattered, for example, among issues of *Przegląd Polonijny*, in a collection monstrously enti-

tled *Employment-Seeking Emigrations of the Poles World-Wide XIX and XX c.* (edited by Celina Bobińska and Andrzej Pilch [1975]) and in *Stan i potrzeby badań nad zbiurowościami polonijnymi* (edited by Hieronim Kubiak and Andrzej Pilch [1976]). Monographs of recent vintage, including Henryk Skok's account of the Poles in Siberia (*Polacy nad Bajkałem 1863-1883* [1974]), have been similarly ignored.

A detailed framework for Murdzek's sections on the partitioning powers vis-à-vis the Polish peasantry could have been found in chapters fourteen through seventeen of Stefan Kieniewicz's classic study, *The Emancipation of the Polish Peasantry* (1969), consultation of which would also have prevented a recurrent error. Alexander's decree of 1861 was not "applied" in Congress Poland in 1863 (pp. 40, 48-50); Russia purchased peasant loyalty with the agrarian reforms of March 2, 1864 and virtually confirmed the favorable terms of emancipation contained in the Red Manifesto of January 22, 1863.

These and other minor imperfections do not materially detract from the importance of a book that demands to be followed by an expanded sequel covering the interwar years and the enfranchisement of the class most disposed to emigrate.

MARIA J. E. COPSON-NIEČKO
Washington, D.C.

STEFAN KORBONSKI. *The Polish Underground State: A Guide to the Underground, 1939-1945*. Translated by MARTA ERDMAN. (East European Monographs, number 39.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. ix, 268. \$16.00.

The Polish underground during World War II was probably the largest and best organized of any in Nazi-occupied Europe. It consisted of an underground government, complete with ministries and territorial administration linked to the Polish government-in-exile in London; an army of 300,000 (the "Home Army" or AK); underground courts that passed sentences that were subsequently carried out; a secret educational system; and a large clandestine press. The underground was predominantly anti-Communist and began to develop in the days following the conquest of Poland by Germany and the Soviet Union in September 1939. As one of the activists who played a role in the underground from its inception until its dissolution in 1945 and as the author of several books dealing with this period of Polish history, Stefan Korbonski is well qualified to have produced the volume under review.

The Polish Underground State, which consists of two

hundred forty-three closely written pages, plus notes, an appendix, and an index, is an almost encyclopedic description of Polish underground organizations and activities during the war. Individual chapters deal with the structure of the underground government, the development and organization of the "Home Army," the diverse nature of civil resistance, the underground press and publications, the very limited extent of Polish collaboration with the Germans, the resistance efforts of the Polish Jews, the impact of the Katyn Forest massacre, the Warsaw Uprising of 1944, the attempted but ill-fated cooperation of the Home Army with the advancing Red armies, the role of the small Communist underground in Poland, and the final months of the Polish underground in 1945.

In addition to a factual description of the complex organizational structure of the Polish underground, Korbonski provides numerous anecdotes that illustrate the nature of the resistance. Particularly interesting are the examples of "diversionary propaganda," known as "Action 'N'." Forgeries of German orders and ordinances, for instance, created much confusion among the occupiers, who tended to respect all official pronouncements. The greatest coup came with the posting, in February 1944, of an order purportedly from General Koppe, chief of police for the *General Gouvernement*, directing all Germans to withdraw immediately from the *General Gouvernement*. Panic ensued, but a swift counteraction by phone succeeded in preventing a mass exodus. More substantial harassment of the Germans was provided by persistent sabotage of the communications systems and of fuel and military storage depots, attacks upon German police and military personnel, and so forth. These and other activities resulted in tying down substantial German forces within the occupied Polish territories.

Korbonski has provided us with a valuable reference work that deserves a place on any shelf devoted to Poland's role in World War II.

HARRY E. DEMBKOWSKI
Alliance College

O. M. RAPOV. *Kniazheskie vladeniia na Rusi v X-pervoi polovine XIII v.* [Princely Domains in Rus' from the Tenth to the First Half of the Thirteenth Century]. Moscow: Izdatel'stvo Moskovskogo universiteta. 1977. Pp. 261. 2 r. 30 k.

O. M. Rapov begins his study of princely domains in medieval Rus' by listing a number of specific questions that he proposes to answer in the book. When did large-scale, private landholding first appear in Rus'? What factors were conducive to its appearance? What were its earliest features? Was

there any relationship between princely and boyar domains; between princely and ecclesiastical domains? And, finally, what were the factors responsible for the disintegration of Rus'—one of the more important states of medieval Europe—into a host of semi-independent and independent territorial units? These questions, Rapov maintains, cannot be divorced from the larger problem of the formation and evolution of feudal society in early Rus'.

Rapov, therefore, turns first to the problem of feudalism. In a lengthy introduction he traces the salient features of the debate among nineteenth- and twentieth-century scholars over this most controversial of topics in Russian historiography. Although Rapov's review of the literature is useful, it is also selective and lacking in balance, taking no account, for example, of the contribution of Western scholars to this debate. Predictably, his own sympathies lie with the Marxist historians who, he contends, have convincingly demonstrated the existence of large, feudal princely domains in Kievan Rus'. Using terms such as "fief," "lord," and "vassal," Rapov attempts to trace the origin of these princely domains and to show that they were indeed feudal in character. He maintains, for example, that by the end of the tenth century the Kievan prince had become a feudal lord who distributed towns and lands as fiefs among his sons, making them his vassals. Even though they are couched in the conventional jargon of Western feudalism, Rapov's arguments are less than persuasive.

By far the most interesting chapters in the book are the concluding two. Here Rapov quite ably shows why princely domains changed hands so frequently and why the Rus' princes switched allegiances so readily (in patently nonfeudal fashion, one might add). He concludes with a look in depth at the princely demesne—its economic base, its social structure, and its daily operations. This last chapter contains some fascinating details about the more mundane aspects of Kievan life.

Better than three-fourths of the book is taken up with biographical and other data, supplemented by genealogical tables in the appendix pertaining to princes from the various branches of the Rurikide line. Straightforward and factual, these often terse biographies read almost like entries from the early chronicles on which they are largely based. Rapov's purpose in assembling this impressive array of information is no doubt to illustrate the political dismemberment of the Kievan state. What he has in fact done, however, is to make available a readily accessible, systematically organized source of information on each and every descendant of Rurik down to the mid-thirteenth century. It is this that makes the book an in-

valuable research tool for the serious student of this confused and turbulent period of East Slavic history.

RUSSELL ZGUTA
University of Missouri,
Columbia

IA. S. LUR'E. *Obshcherusskie letopisi XIV-XV vv.* [All-Russian Chronicles of the Fourteenth and Fifteenth Centuries]. Leningrad: Izdatel'stvo "Nauka," Leningradskoe otdelenie. 1976. Pp. 282. 1 r. 54 k.

Ia. S. Lur'e, one of the leading specialists in old Russian history and literature and an exponent of the comparative, antiquarian-textological method in these fields, has produced an integrated summary of his long and fruitful labors in the study of "all-Russian" (in the sense of Northeastern Russian, or ethnic Great Russian) chronicles of the fourteenth and fifteenth centuries. His study, like O. V. Tvorogov's *Drevnerusskie khronografy* [Old Russian Chronographs] (1975), represents a fine scholarly contribution that will remain for many years to come not only a substantive monograph on the subject but also an important handbook for researchers of that particular period of Russian history.

The book is an outgrowth of more than a dozen articles on Russian chronicle writing of the fourteenth and fifteenth centuries that Lur'e wrote during the eight years preceding the publication of this work. This study, like his other solid work on the ideological struggles in Russian publicistic writing of the late fifteenth and early sixteenth centuries, attests to the author's scholarly erudition, critical approach to sources and professional literature, and ability to painstakingly trace the development of chronicle writing during two centuries of Russian history. Lur'e's work represents the most comprehensive and updated attempt to classify, systematize and produce typologies of the "all-Russian" chronicles of the period in question.

Much of what Lur'e says about the principal official works, such as the Muscovite grand-princely chronicles or the metropolitan chronicles, is not necessarily original, nor does it represent a significant textological discovery. Quite often he follows the established views of the greatest analyst of old Russian chronicles, A. A. Shakhmatov, or his own teacher, M. D. Priselkov (to whom he has loyally dedicated his book), or A. N. Nasonov and other contemporary Soviet scholars. Lur'e also revives and refines a number of Shakhmatov's hypotheses, such as the one concerning the existence of an "all-Russian" Codex of 1448. Regardless of one's views on the matter of the

dating of this hypothetical Codex of 1448—a later date can be proposed as well—Lur'e is correct in asserting that a compiler or compilers of such a multicodex must have worked at the metropolitan's court, and that the codex was a product of ecclesiastical and not secular circles. Generally speaking, Lur'e's treatment of the relationship between the ecclesiastical and secular elements in old Russian chronicle writing and culture is relatively detached. Nor does he attempt to prove at all costs the existence or preponderance of the secular ideology in most of the texts at his disposal.

One could argue with Lur'e about a number of reconstructions he proposes for individual chronicles and about their dating, as for example, his discussion of the hypothetical Codex of 1453, which was originally postulated by A. A. Shakhmatov. Lur'e bases his revival of Shakhmatov's hypothesis on the manuscript GBL M 3271, a manuscript open to many textological questions. (pp. 129-31).

Lur'e's most original and significant contribution is, however, his subtle analysis of the unofficial chronicle traditions of the last third of the fifteenth century. In this area he has succeeded in showing us the variety of authors and editors, as well as the diversity of schools and currents, that existed in Russian chronicle writing at that time, and by doing so, has broken away from certain dogmatic dichotomies and oversimplified designs, which have dominated the study of Russian medieval chronicle tradition for quite some time. His approach converges with more recent trends in the humanities and social sciences, the representatives of which are no longer satisfied with the traditional emphasis on a few great figures or on one or two dominant schools of thought, style, or ideology in a given historical period. Instead, they insist on the necessity of studying neglected phenomena, orientations, and subcurrents, as well as lesser known or forgotten authors and artists, who have also made worthwhile contributions either as precursors or intermediary figures, and who in many cases cannot be easily categorized and classified according to established scholarly conceptions and theories.

Some of Lur'e's major conclusions—that no "all-Russian" chronicles appeared in the Muscovite area before the last quarter of the fourteenth century or that no Muscovite "all-Russian" grand-princely chronicles were compiled before the second half of the fifteenth century—sound quite familiar to historians of that period. Furthermore, his assertion that the official metropolitan chronicle writing was discontinued in the second half of the fifteenth century is open to debate (p. 258). On the other hand, he departs on certain important issues from some old and still

prevailing canons of Russian national historiography. For example, his comments that Muscovite chroniclers were not seriously thinking about the question of Kievan inheritance and the problem of Moscow's unification with Kiev before the middle of the fifteenth century, and that they began to deal with those issues at a later date (pp. 120-21), coincide with my recent findings on the "Origins of the Official Muscovite Claims to the 'Kievan Inheritance'" (*Harvard Ukrainian Studies*, 1 [March 1977]: 29-52). He makes an obvious concession to a national dogma of Russian historiography, however, when he speaks of the alleged nexus between the so-called Tatar yoke and the "all-Russian" attitudes and views to be found in the chronicles (p. 104). His references to the Tatar yoke imply that he has treated it as a historical fact rather than as an ideological concept of Russian political thought and historiography.

In spite of all the impressive qualities that the work under review undoubtedly has, this reviewer, who has followed with considerable interest Lur'e's scholarly production for more than two decades, is left with a sense of apprehension and uncertainty when it comes to a broader assessment of his work's qualitative contribution to the study of Russian chronicle writing. Lur'e's most recent work suggests that he is more of a highly accomplished craftsman in his field than a methodological innovator or a conceptual thinker, whose work, like that of A. E. Presniakov, for example, would open new dimensions in our continuous quest for a deeper and more profound understanding of early Russian history. His study on the *Obshcherusskie letopisi* has a somewhat mechanical and bureaucratic quality so typical for much of the scholarship produced in our time, both in the East and in the West. This particular quality seems to be a reflection of our civilization's present trends, which tend to obliterate imagination, vision, and, what is most disturbing, genuine human talent.

JAROSLAW PELENSKI
University of Iowa

N. A. GORSKAIA. *Monastyrskie krest'iane Tsentral'noi Rossii v XVII v.: O sushchnosti i formakh feodal'no-krepostnicheskikh otnoshenii* [Monastery Peasants of Central Russia in the Seventeenth Century: On the Essence and Forms of Feudal-Serf Relations]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 363. 2 r. 22 k.

Using materials from six monastery archives, N. A. Gorskaia investigates the economic basis of the peasantry attached to monastic institutions in central Muscovy in the seventeenth century. Such peasants comprised about 20 percent of the entire

enslaved population, and the six monasteries that are the chief focus of this book held estates with a registered population of nearly 8,700 households (*dvory*) in twenty-nine different districts at the end of the century. Divided into two parts with five chapters and a brief conclusion, this monograph (evidently a reworked dissertation) successively treats peasant allotments, the leasing of lands outside allotments, the commune's supervision of landholding and land use, and the nature and movement of rents and of the peasantry's obligations to the state. The method is heavily quantitative: fifty-five tables, one of which covers twelve pages, are scattered through the book, which also includes one map and an index of places. Large amounts of statistics are discussed in the text as well, which practice makes for rather dull reading, especially when combined with a turgid and repetitive style (in one passage—pp. 142-48—the same word is first or second in the opening sentences of twenty-three out of thirty paragraphs). Although the author is careful and sensible in using the often fragmentary data, her frequent quotations from the sources—rent rolls, peasant petitions, office correspondence, land surveys—seldom bring the subject to life. We get only a vague idea of what these peasants were like or how they lived, thought, and died. No individual personalities or particular events stand out against the daunting backdrop of rent and price movements. Everything seems flat, abstract, and bloodless. The "corporate feudal landowners" (who are never analyzed) and the "feudal state" are seen as socioeconomic villains, of course, while the dogged and shrewd peasants are heroes as they strive to stave off serfdom and to expand their productive capacities.

After all this painstaking research, laborious compilation of statistical materials, and plodding analysis, the author's conclusions ring hollow. She sees labor dues as dominant by the start of the seventeenth century, but they rose quite slowly thereafter. Money dues and state impositions rose much faster, however, particularly in the last two decades; so the peasants responded by leasing more land, redistributing landholdings and dues, and increasing the size of households. None of these ideas is new or surprising, let alone exciting. In brief, this monograph strikes me as a solid, conscientious monument to misguided and overspecialized professionalism.

J. T. ALEXANDER
University of Kansas

F. M. ALIEV. *Antiiranskii vystupleniia i bor'ba protiv turetskoi okkupatsii v Azerbaidzhane v pervoi polovine XVIII v.* [Anti-Iranian Protests and the Struggle against the Turkish Occupation in Azerbaidzhan

in the First Half of the Eighteenth Century]. Baku: Izdatel'stvo "Elm." 1975. Pp. 229. 1 r. 40 k.

This book presents the story of Peter I's Caspian campaigns in the 1720s against a backdrop of Persian-Ottoman relations in the Caucasus from 1721 until 1735. The author is convinced that the Persian-Ottoman conflict in an area inhabited by Azeris, Armenians, Georgians, and other smaller nationalities produced a pro-Russian orientation, a "profound desire" by these peoples for Russian involvement and even Russian annexation of the area.

F. M. Aliev's account has some value for Western historians of the Caucasus. He provides direct quotations from accounts of these Caucasian events made by Russian agents, merchants, officials, and military personnel. Most of these have not been published and are found scattered in many Soviet archives, including that of the Institute of History of the Academy of Sciences of the Azeri SSR, which has not been used by European or American historians. Papers of Artemii Volynskii and A. I. Lopukhin, the diary of I. Lerkh, and reports prepared by I. Kalushkin all provide interesting new information about the events in the Caucasus during this period.

The inadequacies of Aliev's treatment, however, diminish the book's value. The author has worked in a scholarly vacuum. At the very beginning (p. 12), Aliev shows his ignorance of Ottoman, Iranian, and Western historiography with his charge that non-Soviet historians have refused to deal with the social and economic bases of Ottoman and Persian developments in the seventeenth and eighteenth centuries. One need only mention the studies of Lambton, Barkan, Inalcik, Orhonlu, and Aktepe (the last of which deals directly with Aliev's subject) to show the fallacy of the author's statement. The most recent non-Soviet source he uses was written in the 1930s (Uzunçarşili).

It is Aliev himself who refuses to deal with broad social and economic developments in his treatment: the price revolution of the late sixteenth and seventeenth centuries and its effect on Ottoman administration in the East; the changing patterns of trade, particularly in spices and silk, that altered the economic basis of society in eastern Anatolia and the Caucasus; Muscovy's desire to enter as a full participant in that trade. All are ignored by the author.

Finally, Aliev's claim that the subjection of the peasantry in the Caucasus by Ottoman and Persian notables resulted in that peasantry's desire for Russian intervention just is not born out by his own data. Those Azeris, Armenians, and Georgians who developed a "Russian orientation" were not peasants but were for the most part notables or

merchants. The Muslim peasants chose either Persian or Ottoman sides primarily on the basis of their own religious stance, Sunni or Shi'ite; almost none welcomed the Russians.

The book should be read, however, by historians of this subject, because the reports of Russians in the area provide new information about Russian imperialistic impulses and ventures in the Caucasus. Historians of Ottoman and Persian studies are so few (worldwide) that it is a shame there cannot be greater cooperation between them.

ALAN W. FISHER
Michigan State University

RUSSELL H. BARTLEY. *Imperial Russia and the Struggle for Latin American Independence, 1808-1828*. (Latin American Monographs, number 43.) Austin: Institute of Latin American Studies, University of Texas at Austin. 1978. Pp. xv, 236. Cloth \$14.95, paper \$5.95.

One of the neglected subjects of diplomatic history has been early Russian-Latin American relations. Russell H. Bartley attempts to fill that void with the first comprehensive account in English. Combining the skills of a Latin Americanist with those of a Russian historian, he has collected materials from a variety of English, Spanish, Portuguese, and Russian sources. The result, nevertheless, is a disjointed study that fails to prove the introductory premise: "Two constants in Russia's approach to America were the interrelated interests of commerce and imperial expansion."

Trade between Russia and Brazil did rise impressively in 1808 due to the special circumstances created by the Continental System and British blockade. The extensive detailing of Russo-Brazilian commerce should be more carefully set in the extraordinary circumstances of the time. Only neutral ships could bring supplies and only from neutral ports, and the United States for a time retreated commercially behind its own embargo, a factor that Bartley fails to mention. Bartley's own figures show a dramatic falling off in trade with Brazil after 1816, when the Napoleonic Wars had ended and shipping reverted to a normal pattern.

By that time Russia became interested in the fate of the restless Spanish colonies. In the most convincing section of the book, Bartley presents a balanced treatment of the policy of Alexander I toward the rebelling colonies, but intervention through the Russian ambassador in Madrid, Tatishchev, was directed more toward stabilizing Spain within the concert of Europe than in establishing a Russian position in Latin America. The book, however, sheds very little light on direct Russian contacts with Spanish Latin America,

concentrating instead on relations with the monarchy in Brazil.

The remark of the tsar's envoy, Pozzo di Borgo, in 1817, that "we do not have a single individual of common sense anywhere in the New World," could easily apply to the whole period under discussion. Despite Bartley's disclaimer, even Georg Heinrich von Langsdorff, the celebrated consul in Rio de Janeiro, seems to fit that description as he dabbled in the development and exploration of South America between 1812 and 1828. The book ends with a narrative of Langsdorff's explorations into the interior of Brazil, but the author fails to show how a "Russian scientific expedition" that contained only one Russian was "an indicator of the deeper sources of tsarist New World involvement" (p. 147).

Bartley's main contribution is in extracting useful new material from archives in Madrid, Lisbon, and Rio de Janeiro. More insights might be obtained by a closer reading of British and American records on the nature of Russian activities in this area. The author relies for the Russian side on a few small manuscript collections, on secondary sources, and for as far as it goes (to 1815) on *Vneshnaia Politika Rossii*, a major Soviet edition of documents. He refers in a note to another scholar's use of "the Petrograd archive of the tsarist ministry of foreign affairs." Obviously, he was not able to work there himself. He should know, however, that Petrograd is now Leningrad and the archive is in Moscow. Disregarding a few other minor annoyances—no maps, absence of publication dates in notes, and failure to cite English translations of foreign works—one still wonders if scholarship would not be better served with a couple of articles or with a monograph covering a broader period of Russian-Latin American relations.

NORMAN E. SAUL
University of Kansas

P. A. TIKHMEDEV. *A History of the Russian-American Company*. Translated and edited by RICHARD A. PIERCE and ALTON S. DONNELLY. Seattle: University of Washington Press. 1978. Pp. xiv, 522. \$35.00.

Immediately following the Crimean War, Grand Duke Konstantin proposed that Russia sell its American colonies, administered by the Russian-American Company, to the United States. He based the proposal on the belief that monopolistic companies had outlived their usefulness and on the conviction that Russian America could not be defended in case of war with a naval power. As part of its campaign to counter this proposal, the Russian-American Company commissioned Peter A. Tikhmenev in 1857 to write a historical "survey" of

the company. Tikhmenev was then in the company's employ, having previously served in the imperial navy for a number of years. To assist in this project he was given unrestricted access to the company's archives. The result of his labors was the two-volume *Istoricheskoe obozrenie obrazovaniia Rossiisko-amerikanskoi kompanii i deistvii ee do nastoiashchago vremeni* published in St. Petersburg, 1861–63. The study not only covered the origins of the company, its operations in Russian America, and its relationship with the tsarist government but also the company's activities in the Hawaiian Islands and the role it played in the exploration and settlement of the Amur River region, Sakhalin, and the Kuriles.

Tikhmenev thought of himself not as a propagandist nor as an historian but rather as a compiler; therein lies the value of his work. His more than four-hundred pages of text and fifty pages of footnotes contain an enormous amount of data and include lengthy quotations from original documents, many of which are no longer extant. The small number of documents that have survived from the company's once extensive archives are currently located in the Soviet Foreign Ministry Archives, access to which has been denied non-Soviet scholars. Therefore Tikhmenev's work is an invaluable source for any study of the Russian-American Company and of Russia's eastward expansion.

Richard A. Pierce and Alton S. Donnelly have now made this basic source available in English for the first time in published form. In addition to producing an accurate and readable translation, they have corrected the few errors found in the original and have added citations to relevant works published since Tikhmenev. It should be noted, however, that the lengthy appendixes in the original edition, which contained numerous documents that added immeasurably to the value of the Russian edition and gave it the character of a primary source, have not been included in this English edition. It is indeed unfortunate that the publisher and/or editors did not see fit to include them in this volume. According to the translators' preface, however, there are plans to publish a separate English translation of the appendixes.

MARY E. WHEELER
North Carolina State University

ROBERT L. NICHOLS and THEOFANIS GEORGE STAVROU, editors. *Russian Orthodoxy under the Old Regime*. Minneapolis: University of Minnesota Press. 1978. Pp. xiv, 261. Cloth \$16.50, paper \$6.95.

This collection of essays is the result of an effort to make accessible the present state of Western schol-

arship on the Russian Orthodox Church during the synodal period and to suggest new topics for further study in the field of Russian ecclesiastical history. It originated in a conference sponsored by the history departments of St. Olaf's College and the University of Minnesota and was held in the spring of 1976; both students and scholars were invited to attend.

The relationship between the Romanov government and the church was one of the chief areas of discussion, and in the articles by Alexander V. Muller and David W. Edwards what emerges is an image of a church tied unwillingly to the state. The relationship was not an equal partnership nor was it a symphony of the Byzantine pattern but one that made the church part of the Russian political system, to use the words of Marc Szeftel. John Myendorff also states the case very well: "Russian Orthodoxy was considered [by the Romanov government] not really as a 'church,' enjoying a degree of autonomy, but merely as a body of beliefs shared by the emperor's subjects. . . ." Nevertheless, the Russian church continued to function under these hardships and even contributed heavily to secular developments in the empire, particularly in the fields of education and law. Robert L. Nichols devotes his essay to a discussion of church accomplishments and points out a number of Western views on the subject that now ought to be revised or abandoned. Whether incorrect opinions will be changed depends, of course, on whether they are based on misinformation.

When it looked as though the church would receive an opportunity to free itself from the bonds of state supervision, many serious problems arose, because the structure of the church in the late nineteenth and early twentieth centuries and the disposition of its friends and enemies were not as they had been in 1717. A clerical caste system had developed (largely as a result of government controls), and some of its disagreeable aspects are discussed in the essay of Gregory L. Freeze. In addition, the church hierarchy had lost touch with many facets of parish and even diocesan life, and the question of how to approach a reform brought forth a welter of conflicting opinion. Paul R. Valiere's essay discusses part of this debate as its focus concentrates on the calling of a church *sobor*, the use of the ancient canons, and the restoration of the patriarchate. The excellent essay of Donald W. Treadgold analyzes yet other problems that faced the church, including the challenge of toleration, and also emphasizes some of the healthy features in church life that may have worked to its advantage if the opportunity to operate free of state control had not disappeared so quickly.

The collection also includes an article by James Cracraft on Feofan Prokopovich, the man who did

so much to carry out Peter the Great's reform of the church. It is a gold mine of useful information, although it is unclear why Cracraft wishes to deny the strong Calvinist influence on Prokopovich. The final article, by Edward Kasinec, is a comprehensive bibliographical study of material available for research on the history of the church during the synodal period. An additional bibliography of secondary works in Western languages is included in the volume by Robert L. Nichols and Theofanis George Stavrou. It will serve as a valuable tool for both students and scholars.

Russian Orthodoxy under the Old Regime is an important book. It ought to be read by all those who are interested in Orthodoxy and the development of scholarship on the Orthodox Church.

JOHN D. BASIL

University of South Carolina

MICHAEL HALTZEL. *Der Abbau der deutschen ständischen Selbstverwaltung in den Ostseeprovinzen Russlands: Ein Beitrag zur Geschichte der russischen Unifizierungspolitik, 1855-1905.* (Marburger Ostforschungen, number 37.) Marburg/Lahn: J. G. Herder-Institut. 1977. Pp. xii, 168.

Few minorities attracted more attention in tsarist Russia than did Baltic Germans. Michael Haltzel's excellent and copiously referenced study investigates the Baltic German question as an encounter between Russian reformers during the second half of the nineteenth century and traditions of local self-government in Estonia, Livonia, and Courland. This work, based on a thorough command of relevant printed sources, began as a Harvard dissertation and was translated into German. Unfortunately, it was published without a bibliography. The book, which describes events generally familiar to specialists, is the first to examine the subject in over fifty years and is more than its title suggests. Rather than describing a tsarist assault on class self-government by the Baltic nobility, which never really occurred, it analyzes the broader theme of dismantlement of Baltic autonomy. By attempting to place Baltic russification within the framework of tsarist politics, Haltzel arrives at an important new interpretation of the Baltic German question.

Russification included regulations designed to enforce use of Russian as the language of local administration, measures intended to provide Orthodoxy with every competitive advantage locally, and efforts to standardize Baltic government, court, police, and educational institutions in conformity with Russian practice and to place them directly under the ministries in St. Petersburg. Haltzel's basic point is that these policies resulted

from two complementary forces. The first was genuinely needed reform. Haltzel sees bids to limit Baltic autonomy as instruments for undermining an anachronistic domination of local life by the nobility and wealthy townsmen. This was especially true under Alexander II and was most apparent in reforms of local government, courts, police, and, to a lesser degree, schools. But Haltzel shows that officials went beyond opening local institutions to wider segments of Baltic society. Language edicts and efforts to support Orthodoxy he interprets as attempts by modernizing reformers, particularly in the century's last two decades, to create a national state—a unity within the empire more fundamental than could be achieved by administrative action alone. This was the second force behind russification.

Haltzel depicts russification's implementation clearly and he ventures an explanation of its causes, but this is a facet of imperial bureaucratic history whose sources lie buried in personal and ministerial archives in Leningrad. Regrettably, Haltzel did not use these, presumably because Soviet authorities remain sensitive to research on Baltic Germans by Western scholars.

Reform and the aim of a national state undoubtedly played an important role in russification. Nevertheless, I am not convinced that reform is an appropriate concept for the era of Alexander III or Nicholas II before 1905. Cultural chauvinism, a phrase Haltzel uses in his preface but not later, more accurately describes the atmosphere than does reform, bureaucratic standardization, or a national state. Furthermore, I doubt that the notion of a national state stood within the mental horizon of any tsar or many of the more important officials of the autocracy. Such remarks constitute no fundamental criticism; they suggest only refinements to a fine piece of scholarship.

BENJAMIN L. BENFORD
Tuskegee Institute

N. M. PIRUMOVA. *Zemskoe liberal'noe dvizhenie: Social'nye korni i evoliutsiia do nachala XX veka* [The Zemstvo Liberal Movement: Its Social Roots and Evolution to the Beginning of the Twentieth Century]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 287. 1 r. 80 k.

The most striking feature of this book is the enormous amount of research that went into its composition. Combining substantial new archival materials (from TsGIA, TsGAOR, TsGALI, IRLI, and others) and widely scattered articles from the periodical press with the more frequently used works (Veselovskii, Belokonskii, Petrunkevich, and Shakhovskoi), the author has produced

rich detail on the social and economic status of the gentry participants in the liberal zemstvo movement and on the formation of the group's programmatic demands, and she has added to our knowledge of the devices they used to achieve a coalescence of the movement in 1901 and 1902.

The book's main contribution is in the third, fourth, and fifth chapters: "Zemstvo Deputies and the 'Third Element,'" "Evolution of the Programmatic Demands of Zemstvo Liberalism," "Forms and Tactics of the Liberal Zemstvo Movement." In chapter three the author utilizes archival data on 1,111 zemstvo deputies in twenty-five provinces for 1890-93 to create a statistical profile of the typical zemstvo deputy: a large landowning, educated, hereditary noble with a government service rank and occupying a position in the provinces as an appointed or elected official. A five-page table summarizes these data. From these 1,111 deputies, the author chose 241 as the most active participants in the liberal zemstvo movement for the period of 1890 to 1905, drew the same statistical profile for them, and added data on their publishing record and their later party affiliations. The only major statistical difference is that 77 percent of the latter group, compared to approximately 35 percent for the former, had completed degrees at institutions of higher education. A fifty-three page table providing the details for each of the 241 persons is appended to the volume.

The portion of the chapter on the "third element" is useful, but only introductory to that very important topic, and their role is not adequately developed in the following two chapters.

The author's stated objective is to show how the class base of the zemstvo liberals restricted their program and actions. After a successful analysis of their demands, in chapter four, from the 1880s to 1902, the author names the sins of the liberals as those of omission: unwillingness to distribute their land to the peasantry and inattention to the problems of the workers that came from narrow focusing on agrarian problems.

My only quarrels with this book are that, first, the author purposely excludes the four northern and Ural provinces to inflate the gentry's statistical plurality in the zemstvos, although dominate they certainly did. Second, data on Chernigov province, one of the nine not represented in the sample, could have been obtained from printed materials to compensate to a large degree for the absence of those data from the collection in TsGIA. Third, the book still does not explain the full significance of the class base, if we must primarily study that, upon the program and actions of the zemstvo liberals.

This book is a major contribution to the field. Its research is more significant than its conclusions

and must be read by anybody professing a serious interest in the topic.

CHARLES E. TIMBERLAKE
University of Missouri,
Columbia

MARC SZEFTTEL. *The Russian Constitution of April 23, 1906: Political Institutions of the Duma Monarchy*. (Studies Presented to the International Commission for the History of Representation and Parliamentary Institutions, number 61.) Brussels: Librairie Encyclopédique. 1976. Pp. 517.

This study of the Fundamental Laws of the Russian Empire is the most definitive in the English language to date. It challenges the reader with a veritable Sargasso Sea of origins, history, and interpretation of each article and evaluates the main forces at work under the constitution—emperor, cabinet, State Council, Duma, and political parties. It offers not so much the introduction of new materials as the presentation of detail to shore up concepts heretofore broadly stated. The viability of the system created by these laws has emerged as a most controversial question, and the work addresses itself to that problem.

Marc Szeftel correctly affirms that the eleven-year span of the Russian constitutional system was too brief for definitive judgments. Only the operation of the system provides us clues for its assessment. The author defines the system as one in which emergency laws competed with civil liberties promised but incompletely implemented. In the perspective of a six-century tradition of statist authority, the emperor could use his residual power for fatal mischief until the breaking point was reached in the public mind at all levels.

The constitution, which emanated from the bureaucracy, assured that the emperor remained predominant. It sprang from his absolute authority, and his potential power to retract or modify it weighed heavily in the minds of both the regime and its legislative opposition. Narrowly interpreted, the franchise, legislative jurisdiction, and civil liberties were severely restricted or violated, and the system was illegally modified to conform to the Prussian as against the original German model. This violation of the official rules of the game developed a feeling of frustration that contributed to the revolutionary mood.

Szeftel's detailed presentation of the expanding power of the Duma through skillful exploitation of its jurisdiction represents a major contribution of the study. It realized *de facto* ministerial responsibility through questions arising from its microscopic study of the budget and from its representatives in the special wartime economy committees,

its interpellations and questions, and its power to determine quotas of military recruits. It generated a degree of cooperation from the Foreign Office and the younger bureaucrats in domestic agencies. Its amendments affected land and zemstvo legislation, justice, women's rights, industrial insurance, taxation, and senatorial reform. Social change reflecting entrepreneur attitudes in agriculture, political liberalization of big business, and growing concern for imperial policy worked toward a common perspective in the State Council and Duma—tendencies reflected in the Progressive Bloc. Increased bourgeois representation in the bureaucracy offered hope for accommodation with parliament. The intransigence of the emperor regarding his authority and his sharp rightward turn in 1915 brought the alternative foreseen by V. A. Maklakov should accommodation fail to materialize—the unleashed action of a developing society, *Acheront*.

This series of rambling essays cries out for an index and editing for repetition, syntax, and misprints. The Old Regime somehow retains its authority to December 1917 (p. 156).

ALFRED LEVIN
Kent State University

GEORGE F. PUTNAM. *Russian Alternatives to Marxism: Christian Socialism and Idealistic Liberalism in Twentieth-Century Russia*. Knoxville: University of Tennessee Press. 1977. Pp. xi, 233. \$13.50.

To the victors are dedicated the monographs, particularly in languages other than those whose society is being studied. That has been the case with prerevolutionary Russian society. Studies in English of nonradical Russian thought are few, and George F. Putnam's book is a welcome addition to them. He has written a useful analysis of an important segment of the cultural history of Russia that has generally been overlooked and that should be of interest to historians of ideas in *fin-de-siècle* Europe.

Marxism was at least *pro forma* successful in Russia, therefore many other modes of thought are presented as alternatives to that ideology rather than in their own right. Putnam takes as his area that rich period from the 1890s to the early 1920s—the twilight of the Russian Empire and the revolutionary period. He concentrates upon three major forms of thought, which he describes as: "1. God-seeking, mystical literary speculation on religious and philosophical topics, seeking to delineate a *new religious consciousness*; 2. *Christian socialist thought*, seeking to bind together Orthodox Christian theology, modern idealist and Marxist philosophy and social action; 3. *Idealistic liberalism*, based on rigorous Kantian and Neo-Kantian epistemology and

ethics, while seeking to reconcile both with modern demands for the fulfillment of the many-sided person and the well-being of the masses" (p. 6).

The discussion of the first trend, concentrating upon the meetings and religious societies that developed in Moscow and St. Petersburg between 1901 and 1914, is the least satisfying part of the book. It covers thirty-six of the 176 pages of text, provides lists of topics discussed in various meetings, and briefly analyzes the views of such thinkers and writers as Dimitrii Merezhkovskii, Vladimir Ern, Valentin Svetsitskii, and V. V. Rozanov. In the bulk of the book Putnam discusses the writings of Sergei Bulgakov and Pavel I. Novgorodtsev. By using just one representative of a trend—and both Christian socialism and idealistic liberalism, lacking structure, ideology, and organized adherents, were at best that—Putnam is able to provide a clear discussion of a limited yet vital subject. Sergei Bulgakov, a onetime Marxist, political economist, and Orthodox cleric, characterized in a nutshell, in the title of one of his collections of essays ("From Marxism to Idealism"), the path of the intellectuals in Russia who at the turn of the century became dissatisfied with positivism, Marxism, and undue politicization of society. Pavel Novgorodtsev was a legal scholar whose critique of utopian thinking was popular among the Russian educated classes on the eve of the revolution. Both these thinkers and activists developed their views not so much as an alternative to Marxism as in confrontation with it. The presentation of the ideas of Novgorodtsev is lucid and readable. One could argue that the structure of Putnam's book, the juxtaposition of Bulgakov and Novgorodtsev, presupposes added levels of organizational approach to their ideas, which is not always the case. For instance, Putnam's stress on the role of the "academy" (an infelicitous use of the term for university or secular higher education in Russia) in the thought of Novgorodtsev is not fully justified. A more serious difficulty is the pervasiveness of religious overtones in Bulgakov that is lacking in Novgorodtsev's approach. When Putnam tries to establish a connection between the religious-philosophical societies and Novgorodtsev, his organizational scheme is badly strained.

Putnam tries to present Bulgakov and Novgorodtsev against the rich background of Russian and European thought. That at times leads him to drop names not necessarily central to the discussion, which can be a slight irritant to the reader.

MARTHA BOHACHEVSKY-CHOMIAK
Manhattanville College

B. S. SULEIMENOV and V. IA. BASIN. *Vosstanie 1916 goda v Kazakhstane (prichiny, kharakter, dvizhushchie sily)* [The Insurrection of 1916 in Kazakhstan

(Causes, Character, Motive Forces)]. Alma-Ata: Izdatel'stvo "Nauka" Kazakhskoi SSR. 1977. Pp. 164. 88 k.

Besides an introduction, conclusion, and an appendix of thirty-seven selected documents from Soviet archives, this slim monograph on the 1916 revolt in Kazakhstan comprises three chapters bearing the titles: "Political and Socioeconomic Preconditions of the Revolt"; "The First Imperialist World War and its Influence on Kazakhstan"; and "The Development of the Insurrectional Movement." The first defines the underlying issues (especially the agrarian question) contributing to a revolutionary situation in Kazakhstan on the eve of World War I, links them to developments throughout the Russian Empire, and argues for the existence of a dynamic local Social Democratic organization. The second discusses the material and human cost of the war for Kazakhstan and its role in accelerating the revolutionary crisis and emphasizes once again that the struggle of the Kazakhs was part of the general struggle of all Russian subjects against domestic and foreign oppression. The third analyzes events in various regions of Kazakhstan in keeping with the theses expressed in the earlier chapters.

Those who are familiar with Lowell Tillett's book, *The Great Friendship*, are well aware of the significance which Soviet historiography attaches to the 1916 revolt in Central Asia and Kazakhstan. They also know that the revolt has caused considerable, and often vitriolic, debate among historians within the USSR. Numerous conferences, especially during the early and middle years of the 1950s, and a stream of publications since then have provided the forums for discussion of such topics as the causes of the revolt, the nature of its leadership, ideology, and goals, and above all the relationship between the native rebels and the Russian people. By the early 1960s, over the strenuous objections of some specialists (whose names are noticeably absent from the historiographical survey introducing the book under review), the general outline of an "official" interpretation of the revolt had emerged. B. S. Suleimenov and V. Ia. Basin approach the subject in the spirit of that interpretation. For them, as for the many others who helped to establish the guidelines for discussion and analysis, Soviet historians must view the revolt as fundamentally progressive, revolutionary, and national liberationist. In addition, they must develop the proposition that the events of 1916 in Central Asia and Kazakhstan belong to an empire-wide revolutionary crisis that saw proletarians and peasants, regardless of ethnic background, struggling en masse against tsarism and international imperialism. Where evidence exists to suggest otherwise, it is conveniently ignored.

In all ways, then, *Vosstanie 1916 goda v Kazakhstane* is a thoroughly orthodox monograph written to buttress rather than challenge politically inspired theses. It is not, to be sure, representative of the "new" historiography whose proponents have labored in recent years to expand the parameters of Soviet historical analysis. Yet, as a summary of current Soviet consensus on the subject, this book is of some use to Western scholars.

EDWARD J. LAZZERINI
University of New Orleans

SERGEI STARIKOV and ROY MEDVEDEV. *Philip Mironov and the Russian Civil War*. Translated by GUY DANIELS. New York: Alfred A. Knopf. 1978. Pp. xvi, 267. \$15.00.

Philip Kuzmich Mironov was a Don Cossack of revolutionary persuasion. He was born in 1872, the son of a poor Cossack in Ust-Medveditskaia *stanitsa*, and died in Moscow's Butyr'skaia Prison in 1921, a misunderstood and discredited Bolshevik military commander. From adolescence, Mironov displayed a rare combination of intellect, leadership, determination, and frankness, all of which enabled him to transcend his background and all of which eventually helped land him in difficult circumstances. After completing *gymnasium* study and the Don Cadet Corps, both uncommon achievements for a Cossack of his parentage, Mironov entered active military service in 1902. By 1905 he had attained the rank of Cossack subcaptain for his Manchurian service, but the following year he was stripped of that rank for speaking publicly against the use of Cossacks as military police during the wave of repression following the Revolution of 1905. At the outbreak of World War I, Mironov was restored to officer status, and by 1917, he was wearing the insignia of a lieutenant colonel decorated repeatedly for heroism.

Like other Don Cossacks so familiar to us from the pages of *The Quiet Don*, Mironov returned home late in 1917 to find another front. Following the establishment of Bolshevik power in October, a number of counterrevolutionaries had fled to the Don, hoping—not without some reason—to find a Russian Vendée. What they did not understand was that the Don was hopelessly divided against itself. Fewer than half the population were Cossacks, while the remainder constituted a mixture of non-Cossack peasants and urban elements, none of whom had much in common with the Cossacks and their way of life. Worse still for the counterrevolutionaries, the Cossacks failed to find common cause among themselves. Some gravitated to the emerging White movement, while others, including Mironov, whose views initially cor-

responded with the maximalist faction of the Socialist Revolutionaries, drifted into the Soviet camp. Those who sought neutral ground, initially perhaps a majority of the Cossacks, found themselves inexorably drawn to one side or the other as their brothers reached for their rifles and sabers either to resolve complex questions of political power or simply to protect themselves and their families.

Mironov's mixture of radical politics, personal magnetism, and military experience soon thrust him into the front ranks of those who fought for Soviet power. Some of these same qualities also provoked the jealousy and hatred of potential rivals. After compiling a brilliant military record against Krasnov in 1918, Mironov fell under a cloud in 1919 for going over to the attack against Denikin without orders. Next year, he joined the Bolshevik Party and fully vindicated himself as commander of the Second Cavalry Army while participating in Wrangel's rout from the Crimea. Still, Mironov's successes could not suppress whispers that he was an opportunist, a military specialist, a latecomer of doubtful loyalty. In addition, Mironov's Cossack origins and his outspoken attacks on Moscow's "deCossackization" policy in the Don made him an inviting target for denunciation. Finally, in 1921, while awaiting trial for trumped-up charges that he had incited Cossacks of the upper Don to anti-Soviet rebellion, Mironov was shot in the back in a Butyr'skaia exercise yard.

Mironov was posthumously rehabilitated, but his reputation never emerged unscathed from the barrage of slander and false accusation leveled against him. One of the recurrent themes in the Mironov story is that too many leaders who later became influential (including, apparently, S. M. Budennyi) had a personal stake in allowing the truth to die with Mironov. Why share credit for Bolshevik victory in the south with a dead man? To counter official lassitude and a recurring tendency to view Mironov as a villain, S. G. Starikov, a onetime associate of Mironov and now deceased, and the historian Roy Medvedev collaborated in writing Mironov's biography. They appear to have cleared their protagonist's name, and in the process they have made an important contribution to the growing body of historical literature on the Russian Civil War. Indeed, their whole treatment underscores the need for a comprehensive reassessment of that conflict from the Soviet side. Guy Daniels's translation reads smoothly and is marred only by occasional lapses in the spelling of place names and by a failure to explain some citations that will prove difficult for the non-specialist.

BRUCE W. MENNING
Miami University,
Oxford, Ohio

KENDALL E. BAILES. *Technology and Society under Lenin and Stalin: Origins of the Soviet Technical Intelligentsia, 1917-1941*. (Studies of the Russian Institute, Columbia University.) Princeton: Princeton University Press. 1978. Pp. xiii, 472. Cloth \$30.00, paper \$12.50.

This is an important book because it opens up a new and crucial field in which to examine the nature of the Stalinist system. For a very long time historians felt stymied in approaching Soviet Russia of the 1930s because they were deprived of source material on familiar subjects, especially the formation of high policy. The chaos and terror of the 1930s cut off the rich veins of information that existed for the 1920s. The grandiose events associated with the advent of full-blown Stalinism—collectivization, industrialization, and terror—could, it seemed, only be described and the reasons only guessed at, operating under the dubious assumption that the results explained the purposes. Historians were demoralized by the lack of archives, the paucity of other sources of reliable facts, and the dangers of misinformation spread by official Soviet publications. Most serious work was left to economists and political scientists.

In the last few years this situation has changed radically, and the historical study of Stalinism in the 1930s has leapt forward dramatically. The change has become possible by putting questions about high politics, accessible mostly to conjecture, onto a back burner and looking instead at professional groups and generations. The specific questions and disagreements discussed by professionals, when carefully analyzed, can tell us not only about their immediate concerns but also and even more importantly about the context in which they arose and how they were solved. We do not know enough about Riutin and Kirov and Stalin; such matters remain a secret to those who have access to the closed archives. But the quarrels, for instance, among agronomists, scientists, writers, and jurists, the kinds of appeals to authority which they made, and the manner in which they found resolution, tell us more about the system of power in Stalinist Russia than would a pile of committee meeting minutes. Kendall E. Bailes has undertaken this task for the engineering profession and traces its position from hounded pariah at the beginning of the First Five Year Plan to one of the most prestigious callings by 1941, on the springboard to its current exalted place in Soviet society. The tensions between "expert" and "Red," between cadres formed before and after the revolution, between managers and technicians, between demands from above and fulfillment from below, between diplomas and *praktiki* comprise much of the substance of the pyrotechnics of the

1930s. Bailes has established the parameters of the subject, and his study will be the standard work for some time.

Parts of his discussion deserve special attention: the description of the "Shakhty Affair" of 1928 as an attempt to strangle technocracy in its cradle (this chapter was first published in the *AHR*, 79[1974]: 445-69); the problems faced by indigenous Soviet technological innovation and the tension between native and borrowed technique; the misguided priorities of aviation in the 1930s; the loss and recapture of technical education by curriculum conservatives. One of the most interesting threads of the study is the problems engineers had with worker resentment and resistance from below as well as the more familiar scourges from above. The author makes very effective use of the contemporary specialized press and of Soviet dissertations (though some of his "unpublished" sources have in fact been published).

Some reservations must be expressed. The title of the book is misleading since the focus of the book is on 1928-41 with only a short, if splendid, account of Russian engineering before then, and the period covers only half of the Stalin dominance. Bailes too often tries to elevate what he knows and can demonstrate up to the level of Stalin's personal plans, intentions, and fears, which remain unknown. The problems of Ordzhonikidze as commissar of heavy industry are presented as struggles with the "Stalinists" when surely they are best seen as inescapable contradictions within Stalinism itself. Bailes's use of "class" can only be disputatious, and one might wish for a happier concept, or at least word, than "technostructure" to identify his group. But these are minor compared to the illumination he has shed on an indispensable factor in the formation of Stalinism.

DANIEL MULHOLLAND
Tufts University

GAIL WARSHOFKY LAPIDUS. *Women in Soviet Society: Equality, Development, and Social Change*. Berkeley and Los Angeles: University of California Press. 1978. Pp. x, 381. \$17.50.

Does socialism liberate women? By liberation we mean not merely easing a woman's life but enabling her to be fully equal in society. Gail Warshofsky Lapidus argues that the pursuit of modernization in the Soviet Union has, through a system of sometimes unintended constraints, limited the scope of sexual equality that early on the Soviet regime promised. Her analysis is based on extensive research culminating in thirty-nine in-

valuable tables that demonstrate comparative time budgets; female distribution in the party, in higher education, in the work force, in responsible positions, and in public leadership; and women's wages, aspirations, birth rates, and even their reasons for divorce.

She concludes that Soviet patterns have integrated women into public life on terms that have limited their equality, have given women, but not men, dual roles, and have perpetuated distinctions between male and female work. Lapidus hypothesizes that the twin socialist goals of increased population and women's genuine equality may have proven to be wholly at odds and therefore unattainable. A social science debate, reopening the question of women's role in Soviet society, focuses on how to reverse the decline in birth rate. Proposals range from paying mothers to remain at home to part-time employment. Considerations of women's professional advancement and equality are generally overlooked. The impact of social science analysis on public policy formation falls outside the scope of this study but is the obvious next question. Lapidus anticipates a growing differentiation in male and female roles in part because of Soviet anxiety about the birth rate, in part because of married women's difficulties in acquiring further training to upgrade skills. Women's professional equality has been sacrificed to preservation of her role in the family, hardly the revolutionary blueprint.

Why are women clustered at the lower ranks of professions? A Soviet professor of medicine suggests that women's inability to move to new posts and their temporary departures from work for family priorities that outweigh professional obligations make men seem more reliable and entitled to preference. Lapidus does not pursue the issue, but clearly an unintended consequence of the privileges for working mothers in Russia (as in Eastern Europe) has been to hinder women professionally.

Lapidus suggests that the "conservative" trend, which will widen the differentiation between men and women economically, may be welcomed by Soviet women. Her conclusion supports the assumption of other social scientists that Soviet women are, in fact, relieved that domesticity excuses fuller participation. Why? Surely women's failure seriously to alter priorities may be traced partly to the demise of the feminist Women's Section of the Communist Party (treated in early chapters). Lapidus refers to demands for male participation in child care. Are such demands (as in Hungary, for example) largely from women? Is there a nascent revival of feminism along with conservatism? It would be ungenerous to criticize Lapidus for questions unexplored when she has contributed so much, so well. Meticulous research

and sophisticated analysis make this study outstanding, indeed, the best available.

BEATRICE FARNSWORTH
Wells College

BORIS SHRAGIN. *The Challenge of the Spirit*. Translated by P. S. FALLA. New York: Alfred A. Knopf. 1978. Pp. xv, 262. \$10.00.

Boris Shragin was a Soviet art scholar, a convinced Marxist, and a member of the Communist Party in his previous existence, and he is one of the ideologists of the liberal dissident movement, an émigré, and a Russian existential thinker in his present existence. The genre of his book is closest of all to the classic Russian confession. As in the majority of famous Russian confessions (for example, in Petr Chaadaev's *Philosophical Letters*, in Alexander Herzen's *My Past and Thoughts*, or in Nikolai Berdiaev's *Self-Awareness*), the author is confessing not so much his own sins as the sins of his country and people.

Of course, Shragin's personal experience forms the basis of the book—his spiritual path from membership in the Communist Party to dissidence, from Marxism to existentialism. But the reader cannot escape the feeling that the author speaks not so much in his own name as in the name of a definite social stratum, the one which it seems was buried along with pre-Soviet Russia, the one whose existence many (primarily Alexander Solzhenitsyn) deny in contemporary Russia. In a word, he speaks in the name of those who are traditionally called the intelligentsia. And this, from my point of view, is the most interesting and significant thing about *The Challenge of the Spirit*: the Russian intelligentsia—in ways unknowable and mysterious—is being reborn from the ashes, having survived "war communism," total terror, and the GULAG Archipelago. Shragin's book also testifies to the commotion of feelings in which the revitalized intelligentsia now finds itself, to the continued shakiness of its intellectual positions, and to its uncertainty in the complicated spiritual search that it is renewing in the second half of the twentieth century—after a long dark intermission.

The book has five chapters: "History at a Standstill," "Nostalgia for History," "State Serfdom," "The Challenge of the Spirit," and "The Dissidents." The chapter titles themselves show that the book would be a collection of rather ill matched essays were they not connected by the unity of the author's fate and philosophical conception. The crux of this conception is, roughly speaking, a denial of the category of time in Russian culture. Historical time, of course, is being spoken of (or the historical memory of the nation).

"In the popular mind there is no sense of historical continuity," says the author in the section "The Abolition of Memory." "A sense of historical continuity [is lacking]. . . . This . . . is the Achilles' heel of the Soviet mentality, and the traditional Russian one in general," he repeats in the section "A Spiritual Malady." In other words, the Russian people unfortunately are incapable of learning from history and therefore each generation has to begin everything from the beginning. Thus, since spiritual liberation on a national scale as a result of conscious action of social forces in Russia is impossible, nothing else remains but the path of individual self-perfection—a lonely salvation of the soul, so to speak. When there are enough enlightened souls, the spiritual liberation of the nation will occur by itself. Shragin proposes precisely this alternative (following his present teacher Berdiaev). The sole difference is that Berdiaev advanced his conception at the memorial feast for the pre-Soviet intelligentsia, and Shragin advances it in the years of the initiation of the Soviet intelligentsia. One was a witness to the end. The other is a witness to the beginning.

Naturally, Shragin's conception reflects the collapse of hopes for the liberalization of the country, which is carefully documented in this book, the impossibility of direct political action in an authoritarian system, and despair at the sight of sacrifices made in vain attempts at struggle—in short, it reflects what Shragin's former teacher Lenin called "defeatism."

The Stalinist GULAG, however, was not at all the first tragedy of the Russian intelligentsia. And this is not their first rebirth. Did not the same thing occur after the epochal defeat of the Decembrists? And did the intelligentsia not prove capable of inspiring great reforms after the dark years of political terror under Nicholas I? The same history testifies that it is hardly possible to stop the intelligentsia's spiritual search, once it has begun, by any means short of a new GULAG and physical annihilation. And besides that, they are still learning something on their journey to Golgotha. Just one example: the pre-Soviet intelligentsia (in the overwhelming majority) was infatuated with revolution; in it and only in it did they see the path to the salvation of the nation. The Soviet intelligentsia (in just the same overwhelming majority) hates the revolution. Their contemporary prophets may disagree among themselves about everything, but if anything unites them, it is precisely their hatred of the revolution. Does this not mean that over the last half century the Russian intelligentsia has learned something? Moreover, does not this hatred for the revolution unite Shragin with his irreconcilable opponent Solzhenitsyn? Thus, Shragin's book it-

self—in spite of its thesis—involuntarily testifies to the ability of the Russian intelligentsia to learn from history.

ALEXANDER YANOV
University of California,
Berkeley

NEAR EAST

A. W. KAYYALI. *Palestine: A Modern History*. London: Croom Helm; distributed by Biblio Distribution Center, Totowa, N.J. 1978. Pp. 243. \$21.50.

As the prospects for peace between Israel and at least some Arab states increase, the political position of the Palestinian Arabs becomes increasingly important. Historical antecedents are being examined so as to understand the ability and desire of the Palestinians to accept or reject a partial settlement of their grievances as well as the methods by which their leadership has been selected and legitimized. This survey of Palestinian Arab nationalism from the 1880s to 1939 provides a useful summary of the British records on these subjects.

A. W. Kayyali emphasizes the early emergence of Arab opposition to Zionism before World War I and its strengthening afterward. The delay in establishing the mandate in Palestine, a desire for Arab national independence, fear of Zionist immigration, and increasing dispossession of Arab peasants by Jewish settlements created deep anti-British and anti-Jewish feelings among the Arab Palestinians after 1917. Although the large Arab landowners and political notables led the opposition to the British, they also cooperated with the mandate government, Kayyali argues, so as to maintain their influence, wealth, and official positions. In the 1920s and early 1930s the cooperative attitude of such leaders as Hajj Amin al-Husayni secured few compromises from the British, who insisted upon the creation of a Jewish national home in Palestine. According to Kayyali, the notables weakened the cause of Arab independence by their internal disagreements, timidity, and selfishness.

Arab boycotts directed against Zionist exclusivism and British policies were caused by popular pressures as were occasional large-scale demonstrations and riots. Increasing Jewish immigration in the 1930s caused a general, spontaneous revolution to break out among the Arab peasantry; the notables' leadership of the 1936-39 revolt came late.

Although the British crushed the Palestinian revolt by 1939, the Arabs as a result of their experiences since the 1880s were bitterly opposed to Zi-

onism, reluctant to accept any compromise short of majority rule in an independent nation, and convinced of the duplicity of the British. The ineffectuality of the traditional leadership of the Palestinian Arabs had also become clear by 1939.

In presenting the history of the Palestinian Arabs, Kayyali ironically relies primarily upon British archival sources; only those secondary sources printed before 1971 are examined. There is relatively little new to be gained from the Colonial Office archives. Other than the detailed and strong criticism of the Arab notables, which is predictable from a member of the Arab Liberation Front such as Kayyali, no new analyses or striking revisions are advanced. The work is marred by a nearly complete disregard of Zionist argumentation, for example, two sentences alone are devoted to the Nazis and the persecution of the Jews in Europe.

Although the modern history of Palestine is clearly dominated by the struggle for political power between Zionists and Arabs, to ignore all other aspects of history as Kayyali does is to deny the multifaceted nature of human experience. Education, technological innovation, cultural creativity, theological speculation, economic development, and the influence of Western ideologies upon Palestine are only a few aspects of the modern history of Palestine that would lend depth to an understanding of this crucial phase of recent history.

WILLIAM OCHSENWALD
Virginia Polytechnic Institute

PHILLIP J. BARAM. *The Department of State in the Middle East, 1919-1945*. Philadelphia: University of Pennsylvania Press. 1978. Pp. xxiv, 343. \$27.50.

A welcome addition to the growing body of literature on American relations with the Middle East is Phillip J. Baram's *The Department of State in the Middle East, 1919-1945*. Using recently opened materials in the National Archives, interviews with participants, and the papers of key officials, Baram extends the pioneering work of John DeNovo into the years 1939-45. Baram's study, copiously researched, clearly argued, and straightforwardly written, describes the state department's remarkably consistent affinity for Sunni Arabs. It comes as no surprise in the wake of recent books by Arthur Morse, Henry Feingold, and Saul Friedman that the department also consistently opposed Zionist aspirations.

Baram finds constant the department's hope that support for Sunni Arab nationalists committed to gradual reform would best serve American interests in the Arabic-speaking Middle East (Egypt, Arabia, and the fertile crescent). He iden-

tifies American interests as (1) the enhancement of American influence generally and (2) an open door for American businessmen, especially oil operators. The department thus encouraged bourgeois nationalism in Syria, Lebanon, Iraq, and Egypt while supporting the monarchy in Saudi Arabia. In Palestine the department worked to thwart Zionist aspirations, as it discouraged assertions of minority rights by such other groups as Kurds, Assyrians, or Maronites. The author thus fits the state department's negative response to Zionism into a general regional framework.

World War II, to which Baram devotes the most attention, created a fluid situation that the department exploited. Convinced that French colonialism failed to respond to Syrian or Lebanese nationalism and bolstered in its Francophobia by the French capitulation in 1940, the department worked successfully to end French rule in Syria and Lebanon. Since British colonialism seemed more benevolent, as evidenced in their voluntary termination of the Iraqi mandate, since joint war plans assigned operational responsibility for the Middle East to the British, and since Palestine in particular could become explosive, the department acquiesced in continued British control over Palestine, Egypt, and Iraq. After the war, however, the department expected to diminish British influence and expand American influence in these areas as well. In Saudi Arabia, where British and American interests in oil and geopolitical position clashed most directly, the state department succeeded in establishing American hegemony.

Valuable as the work is, one shortcoming deserves attention. Baram claims that the state department's middle management for the Middle East, the dozen men who made Middle East policy, shared "a very similar social and intellectual background" (p. 69) that "bordered on country-club homogeneity" (p. 7). Elsewhere he calls them "insulated and inbred, elitist and of the upper middle class" (p. 87). His evidence, drawn primarily from departmental biographies, is limited to dry lists of colleges attended and official posts held. These data do not confirm any consistent socioeconomic pattern among Baram's dozen policymakers. He postulates homogeneity without proving it.

Baram effectively delineates the state department's attitudes and policies toward the Arab world and thus aids our understanding of subsequent events. Because the department assumed the future lay with bourgeois nationalism under benevolent American leadership, and because it so blithely dismissed Zionism as "chimerical" (p. 246), it was blind to the realities of the postwar world, when Israel would become a nation supported by the United States, and revolutionary

Arab nationalist would sweep away tradition in Egypt, Syria, and Iraq.

RICHARD PFAU
Dickinson College

ÖMER KÜRKÇÜOĞLU. *Türk-İngiliz İlişkileri (1919-1926)* [Turkish-English Relations, 1919-26]. (Ankara Üniversitesi Siyasal Bilgiler Fakültesi Yayınları, number 412.) Ankara: Ankara Üniversitesi Basımevi. 1978. Pp. 350. 40 TL.

Modern Turkish historical scholarship on Turkish-British relations is only a quarter of a century old, having gotten under way in serious fashion in 1953 with Akdes Nimet Kurat's study on the origins and development of those relations during the period 1553-1610. Ömer Kürkçüoğlu's work, primarily a political and diplomatic history of the post-World War I era from 1919 to 1926, is a welcome addition to what appears to be a growing body of scholarship.

Researchers concerned with the immediate post-World War I years that witnessed the demise of the Ottoman Empire and the birth of the Turkish republic are handicapped by the unavailability of the Turkish documents. This hardship is visited equally upon native Turks and foreigners. As a result, even Turkish scholars are compelled to rely upon Western documentation. Kürkçüoğlu uses British diplomatic sources to good advantage, but his secondary sources are limited to works in English and Turkish. These are not the sorts of materials from which brilliant new insights can be developed. Kürkçüoğlu's contribution, rather, is to present a swiftly flowing narrative that follows the course of Turkish-British relations from the late eighteenth century to 1926.

Kürkçüoğlu has divided his work into three large sections. The first deals with the period from the mid-eighteenth century to the outbreak and conclusion of World War I. Students of the Eastern Question will not find much here to detain them from plunging on to the second and third sections. Section two deals with the years of the Turkish struggle for independence, 1919-22. The concluding section covers the remaining four years, 1922-26, during which Mustafa Kemal Atatürk was able to convert Turkish-British relations from an adversary relationship to one of mutual respect.

The heroes in this drama are Mustafa Kemal and the Turkish nation. Mustafa Kemal's peace program was multipronged. He had to undo the inequities of the postwar settlement, gain the respect of the West, on the battlefield if necessary, and establish complete and full independence and sovereignty for Turkey and the Turks. It was then

and still is a tale of epic proportions. Kürkçüoğlu retells it well.

NORMAN ITZKOWITZ
Princeton University

MUHAMMAD MORSY ABDULLAH. *The United Arab Emirates: A Modern History*. New York: Barnes and Noble. 1978. Pp. 365. \$21.50.

Muhammad Morsy Abdullah's opening sentence indicates the thrust of his book: "The British first came to the Gulf at the beginning of the seventeenth century." Despite its title, *The United Arab Emirates* is as much a history of British involvement in the Persian Gulf as it is of the emirates themselves. Each of the three principal sections—British interests, internal changes, and Trucial relations with neighboring states—deals largely in the traditional currency of Gulf studies: Foreign Office, India Office, Admiralty, and local British Residency reports and opinions. Morsy's focus is explained not only by Britain's long-held role as paramount power in the area, but also by the fact that the book is based on his Cambridge thesis, "Britain and the Trucial States between 1892-1939." Morsy has made use of French, Turkish, German, and American archives, as well as his own quarter-century's professional experience on the scene, all of which contribute to the book's utility, but it is still largely Gulf history through the eyes of Whitehall.

Unfortunately, Morsy's expertise is not always used to its best advantage. Too often his account is merely uncritical recapitulation of the chronological flow of events. This is particularly the case in the rather skimpy treatment given to recent developments; would that every developing area had the trouble-free progress outlined for the emirates, and most notably Abu Dhabi. Morsy is a careful and competent scholar, but his fondness for Abu Dhabi, where he serves as director of the Centre for Documentation and Research, is clear. Only occasionally do hints come through of the larger problems that deserve fuller consideration. The effects of the decline of the pearl industry, or the interwar depression, or the sudden incentive to lesser sheiks to declare their independence and thus share the bounty of oil royalties, all have to be winnowed from the chroniclelike data. The few pages that are devoted to economic and cultural change are tantalizing, and it is to be hoped that Morsy will enlarge on these topics in his future oral and archival research. Even the larger concerns of British policy are sometimes hidden. For example, Britain's desire to placate Reza Shah of Iran, most notably in the question of ownership of strategic Gulf islands, rather than risk jeopardy to

Anglo-Persian oil interests or the growth of Soviet influence at Teheran produced a countervailing decline of prestige along the Arab Gulf coast. Morsy knows this, but his separation of the issue of the islands from British coastal policy leaves the interconnection obscure.

The last half of the book is devoted to the frontier problems between the Trucial States and Saudi Arabia, Iran, and Muscat, in addition to interstate boundary issues. On these subjects, Morsy is authoritative, and his knowledge of the tribes and their genealogies unrivaled. The value of the work is further enhanced here by Morsy's own involvement in the mapping of Abu Dhabi. In short, although the book is not quite the comprehensive study implied by its subtitle, it does offer much valuable detail on the Trucial Coast in the era between the conclusion of Britain's exclusive treaties in 1892 and its withdrawal from the Persian Gulf in 1971, and to that extent is an important contribution to Gulf studies.

BRITON C. BUSCH
Colgate University

AFRICA

MEZRI BDIRA. *Relations internationales et sous-développement: La Tunisie, 1857-1864*. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 102.) Uppsala: University of Uppsala; distributed by Almqvist and Wiksell, Stockholm. 1978. Pp. 194. 71 KR.

The bey of Tunis in 1857 was obliged, under British and French pressure, to proclaim a charter of rights similar to that of the famous Ottoman Hattı Şerif of Gülhane (1839). A constitutional regime, promised at that time, came into existence in 1861 (the first in the Arab world). Three years later, in 1864, a frantic doubling of taxes sparked a revolt that came within a whisker of toppling the regime. The constitution was suspended and Tunisia's brief experiment in constitutional monarchy came to a close.

In a thoughtful and well-conceived book Mezri Bdira treats this period within the conceptual framework of modernization theory. The author's thesis is that the Tunisian ruling elite was not unaware of the challenge it faced from Europe. The "westernizers" or "reformists" among that ruling elite sought to adapt the Western model of government and society by implementing a not unrealistic program of state-directed development. They hoped to avoid the suffocating political embrace of any single European power (especially Britain and France). The cumulative European

pressure was, however, too much. Efforts to rein in excessive privileges of foreign residents, to establish modern credit facilities (the Anglo-Tunisian bank project), and to avoid wasteful development projects or onerous loans were all frustrated by the European politicoeconomic power structure (with France in the vanguard).

Those familiar with the nineteenth-century history of the Ottoman Empire or Egypt or Morocco will find this thesis familiar. Yet Bdira is ploughing new ground. He uses developmental theory judiciously but not slavishly. He has used Tunisian sources (especially the Tunisian National Archives) more effectively than his predecessors.

And he offers at long last a rebuttal to Jean Ganiage's *Les origines du protectorat français en Tunisie, 1861-1881* (1959), for Ganiage's meticulous mastery of the European sources does not suffice to veil his supercilious and often inaccurate interpretation of pre-Protectorate Tunisia.

Bdira's challenging thesis (crisply stated in a six-page conclusion) must, however, still be viewed as tentative. More needs to be done to establish the fiscal position of the Tunisian government in the 1850s and 1860s. Bdira's classification of the ruling elite into two groups—westernizers and *ulama*-led traditionalists—neither of which was completely right or wrong in their analysis of the situation is very promising, but he does little more than scratch the surface. Nor does the author dig deeply into the inner workings of the short-lived constitutional system. The author is, however, on a useful new track.

L. CARL BROWN
Princeton University

OMONIYI ADEWOYE. *The Judicial System in Southern Nigeria, 1854-1954: Law and Justice in a Dependency*. (Ibadan History Series.) Atlantic Highlands, N.J.: Humanities Press. 1977. Pp. xiii, 331. \$14.75.

The rather bland title of this book conceals the most informative work available on the legal history of Nigeria and perhaps the most original work yet to appear in the field of modern African legal history. Omoniyi Adewoye, senior lecturer in history at Ibadan University, displays a rare combination of expertise in the law with a fine sense of historical analysis. Just as impressive is a precise command of the English language, beautifully controlled, spiced with wit and elegance, which has turned a first-class academic monograph into a piece of literature that is a pleasure to read. This achievement is all the more impressive when the structure of the author's sources are examined; Adewoye has cast his net wide and deep among collections of private papers, colonial office corre-

spondence, the Nigerian National Archives, Nigerian court records, and official publications and newspapers. Yet none of this massive and detailed documentation interrupts the steady focus on general analysis, even where illustrations of individual cases are used extensively.

Adewoye views the application of law as "a technique of social ordering" (p. 1) and thus intrinsically a part of political, and even more of social, history. Its main purpose in Nigeria was the stabilization of colonial rule over the African population, which, once achieved, could lead to the progressive integration of the people into wider economic systems. An excellent first chapter outlines the complex dilemmas that arose from the juxtaposition of a British court system with that of precolonial society. Notions of law and justice were simply one element of the fundamental cultural differences between the two societies. This theme runs through the whole book, which conveys a vivid sense of the inevitability of the decline of customary and traditional law, however the British might have attempted to prop it up by limiting and modifying British traditions or to exclude their influence from the "native courts" that were developed and sometimes invented under the indirect rule system.

The author tells the story of the development of the British-imposed judicial system from its origins in the "courts of equity" in 1854 to the regionalization of the system in 1954. While there is much discussion of the argumentation that went on among educated Africans, colonial civil servants and judges, and the colonial office in London, Adewoye constantly compares theory with the system in action. In the process the work spills over into social history, exposing much new material. The discussion of the Nigerian legal profession is especially valuable, not so much for what it says about qualified lawyers as for the fascinating accounts of the paralegal professions that grew up, partly as a response to British policies designed to exclude African barristers from the lower courts. The 1920s and 1930s saw the emergence of "court champions," professional writers of petitions and contracts, court clerks, and interpreters. They ranged in ability from near-illiterates to those who today might well deserve to wear judges' robes and in morality from parasitic touts and loan-sharks to patriotic radicals.

Adewoye studiously avoids any temptation to sit in judgment over the judges or their critics. He has no villains or heroes; his many vividly drawn characters are simply men of their time and circumstances. Only on the last two pages does he abandon that detachment, facing the present situation in Nigeria as it prepares to move back to democratic constitutionalism. He pleads for the rule of

law and an independent judiciary as viable and superior alternatives to force, thus revealing himself as part of the process he has been discussing.

JOHN FLINT
Dalhousie University

FRED I. A. Omu. *Press and Politics in Nigeria, 1880-1937*. (Ibadan History Series.) Atlantic Highlands, N.J.: Humanities Press. 1978. Pp. xi, 290. \$16.00.

The role of the newspaper press in the political and cultural development of modern West Africa is an attractive and important theme. It has not been altogether ignored by historians, commonly receiving some discussion in standard treatments of the development of nationalist politics, featuring more centrally in such general works as those of K. A. B. Jones-Quartey and in studies of public opinion concerning individual political issues like S. Asante's study of the West African response to the Italian invasion of Ethiopia, and, naturally enough, in memoirs such as Azikiwe's *My Odyssey*. Fred I. A. Omu has produced, however, the first reasonably coherent historical treatment of the development of the press in any single West African country for the first phase of its history. He begins with the predecessors of the Lagos newspapers in other West African territories, carries his story through the foundation of Azikiwe's *West African Pilot* in November 1937, and closes with a brief and mildly despondent epilogue on subsequent developments.

His account of the institutional development and especially of the financial history of the papers is as useful as it is (inevitably) patchy. He also provides a considerable amount of documentation on the foibles of different editors and their institutional foes, particularly the successive colonial governors. He is somewhat less successful in analyzing precisely how the position of the press displayed the intrinsic anomaly of the colonial state as a structure of control. To most governors the press seemed, as Governor Freeman put it as early as 1863, "a dangerous instrument in the hands of semi-civilized Negroes" (p. 173), and one which it was evidently desirable, if at all possible, to keep out of the hands of many of them. Most Colonial Office officials, by contrast, preferred that freedom of publication should be regulated, as in the metropolis, by common and statute law rather than simply subject to the arbitrary whim of the governor, a preference that caused Lugard, for example, well-deserved distress during his period of office. In relation to such issues the main outlines of Omu's analysis are less illuminating than they could be, despite the vividness of some of his documentation. But he succeeds reasonably well in

characterizing the varying roles of the press in articulating and in part even forming Lagos society's response to colonial rule, as a set of instruments in local factional conflict, and in some cases as the organs of institutionalized political parties. All this is useful enough.

The book possesses, however, a number of severe defects. It is very discursively written, a trifle callow in tone in many places, and dramatically poorly subedited—full of typographical errors, some of which obscure or even alter the sense of the sentences in which they appear. Evidently a largely unrevised doctoral thesis of by now fairly ancient vintage, the work fails to relate its analysis to (or indeed to mention) virtually any of the scholarly studies of the political and social history of Lagos and its hinterland in this period that have appeared in the last seven or eight years. Virtually all, for example, of its predecessors in the Ibadan History Series that are concerned with the history of Yorubaland appear in its bibliography as unpublished doctoral theses. In these ways it marks a disappointing departure from the high standards set by earlier works in the series.

JOHN DUNN
King's College,
Cambridge University

UGBANA OKPU. *Ethnic Minority Problems in Nigerian Politics: 1960-1965*. (Studia Historica Upsaliensia, number 88.) Stockholm: Almqvist and Wiksell International. 1977. Pp. 191.

Ugbana Okpu's primary aim in writing this book is to provide a comprehensive and systematic analysis of the role of ethnic minority groups in Nigerian politics up to 1965—the title is misleading since over half of the book is devoted to the colonial period—with a view to assessing their contribution to Nigerian political history and the relationship between their problems and political instability. The study fails sadly to fulfill this ambitious aim. It is neither comprehensive in its analysis nor systematic in its approach. On the contrary there is a preoccupation with the issue of the creation of states. In an attempt to escape a "conspiracy" of neglect of the role of minority groups in most works on Nigerian history, Okpu goes to the other extreme of neglecting anything that interferes with his basic theoretical premise: a major incompatibility within the Nigerian polity between minority groups seeking their own states and major parties refusing to create them has been the cause of political instability in Nigeria. Moreover, at no stage is there any systematic substantiation of that premise. A chapter on "New States and Political Instability" makes no reference to the

extensive general literature on the subject and provides a strictly constitutional analysis. Constitutional engineering may be an important ingredient in providing for political (in)stability, but by itself it is unlikely to guarantee political stability. Although the book contains much information, there is little to encourage the reader to regard it as authoritative. Okpu complains about the lack of conceptual clarity in other works but provides four pages (pp. 10-14) of definitions that beg questions about much that follows. For example, "ethnic" and "national" are used synonymously, but ethnic separatism is used to refer to the promotion of ethnic interests to the detriment of national interests! An ethnic minority group is subjectively defined (to ensure maximum conformity with the author's premise?) as one which "does not constitute a majority in a region," "which does not have access to political power," and "which considers itself dominated, discriminated against, neglected or oppressed by the major ethnic group in its region" (p. 11). There are many other examples of unnecessary conceptual confusion.

The chapters that follow on the origins of ethnic minority problems, the policies of the major political parties on such issues, the ethnic minority parties, and new states and instability provide an uneven and fragmented analysis, marked by errors of fact and interpretation too numerous for the reviewer to mention. In short the book has little to recommend it to potential readers.

D. G. KERMODE
Liverpool Polytechnic

AUDREY WIPPER. *Rural Rebels: A Study of Two Protest Movements in Kenya*. New York: Oxford University Press. 1977. Pp. xiv, 363. \$17.50.

Audrey Wipper's two protest movements are the Mumbo cult and the Dini ya Msambwa. The first arose in 1913-14 among the Luo people of Western Kenya and spread to some clans among their southern neighbors, the Gusii, where it appeared sporadically throughout the colonial period. It was anti-European and anti-Christian. Its members believed that there would soon be a cataclysmic upheaval that would destroy the Europeans and lead to a utopia where there would be no death, famine, or work. Mumboites tried to hasten the arrival of the millenium by symbolic resistance to British rule—by refusing to wear European clothes or to eat European food and by practicing traditional magical and sacrificial rites. Despite government fines and deportations, Mumboism and its offshoots continued to surface until Independence.

The Dini ya Msambwa also arose in Western Kenya, among a sub-group of the Luyia, and was

based on a similar appeal to a glorified African past. But it appeared later, in 1943, and lasted longer—it has underground adherents today. It was also much more important, and Wipper accordingly devotes the greater part of her book to it. Compared to the peak number of a few hundred Mumboites, the DYM had literally thousands of members. More significantly, it encouraged violence—arson, assault, and civil disturbances like the Malakisi Riot in 1948 and the much-publicized “Kololo Affray” early in 1950 when twenty-nine Africans and three Europeans were killed. Mumboism had many leaders over the years. In contrast, the DYM centered on one man, Elijah Masinde, a prophet-messiah figure to his followers, a mentally unstable demagogue to the British administrators and their African successors.

From my own probings into post-World War II Kenya politics and around the Luyia area, I would argue that Wipper makes the DYM seem more important than it was. Except for some adherents among the Pokot and the Nandi, it was a local Luyia organization with little widespread or lasting impact. It was much more the creation of Masinde (who, Wipper admits, built a career on rebelling against all forms of authority, African or European) than a nationalist movement, composed of “guerrillas,” as she claims. There was certainly no shortage of grievances against the colonial regime among the northern Luyia, and it is true that because of Masinde’s static-electricity kind of personality the DYM picked them up. But he used them to build a personal following and to harass the administration, not to further the nationalist cause. The DYM was deliberately parochial; it had no ties to other protest groups. Masinde’s ego would not allow them.

This overweighting of the DYM’s significance no doubt results from Wipper’s close involvement with it. She has made it her specialty and clearly dominates the field, as her treatment of the other scholars who have ventured onto her territory shows—most of them wind up with their toes pointing skyward. Granting her understandable bias, Wipper has written a book that should interest anyone concerned with African protest against colonial rule.

JOHN SPENCER
Middlebury College

J. MUTERO CHIRENJE. *Chief Kgama and His Times, c. 1835–1923: The Story of a Southern African Ruler*. London: Rex Collings; distributed by Rowman and Littlefield, Totowa, N.J. 1978. Pp. 140. \$11.50.

The Ngwato are one of the select groups of South African peoples who retained autonomy through

the ravages of the Difaqane wars; the introduction of Christianity; the intrusion of European settlers, traders, prospectors, and imperialists; and the pressures for unification of all South Africa. Their chief, Kgama III, played a unique role in that his life spanned all those events, including the reconstruction after the wars. This fact justifies his being written about twice during his lifetime and several times since. J. Mutero Chirenje’s decision to try once again is based on his contention that Kgama should be regarded as a pragmatist who responded to forces at work in his land in ways that would best serve the interests of his people to enable them, and him, to survive, and not just be regarded as a slavish adherent to Christianity.

In the short space of one hundred and nine pages Chirenje outlines the chief’s relationships with his father, brothers, and sons, with neighboring Tswana and Ndebele chiefs, with encroaching Boers and British traders and speculators, with the colonial government and British protectorate officials, and with the London Missionary Society and the later separatist preachers.

Because previous biographies emphasized his conversion to Christianity, Chirenje is at pains to show that Kgama acted independently of the church and opposed the missionaries when their actions countered his ambitions. Though his approach seems valid, the effect is to make Chirenje’s account more a reaction to other works than a coherent account of Kgama’s life.

The reader of this biography cannot avoid making comparisons with recent biographies of rulers of the other protectorates of South Africa: Moshoeshe of Lesotho and Sobhuza II of Swaziland. Moshoeshe has been considered by Leonard Thompson and Peter Sanders, Sobhuza by Hilda Kuper. Though Chirenje is the only African among those authors, his volume is the most superficial since he depends upon a limited range of sources. Sanders and Thompson both effectively incorporated oral interviews with descendants of Moshoeshe and other chiefs of Lesotho, even though their subject died much earlier than Kgama. Sanders effectively used extensive vernacular writings, including praise poems. Kuper infused her writing with ethnographic analyses as well as personal interviews with the aged king. Chirenje, in contrast, depends entirely upon government and mission archives and contemporary newspapers, which, with one minor exception, are in English. Because of this limitation of sources and the extreme brevity of the treatment, Chirenje’s book offers only an outline of Kgama’s career and of the forces at work around him and leaves but a shadowy impression of the character and personality of a great chief.

WILLIAM F. LYE
Utah State University

L. H. GANN and PETER DUGNAN. *South Africa: War, Revolution, or Peace?* (Hoover International Studies.) Stanford: Hoover Institution Press. 1978. Pp. xii, 85. \$5.95.

L. H. Gann and Peter Dugnan's informative, succinct, and highly readable monograph *South Africa: War, Revolution, or Peace?* is a thought-provoking addition to the burgeoning literature on United States-South Africa relations. The basic theme of the study is that much of the current thinking on South Africa in the United States is seriously misguided; that South Africa is wrongly singled out as a pariah state by an international community corrupted by double standards; that South Africa's strategic situation and mineral resources are factors that must influence policy makers; that military intervention or an armed insurrection are unlikely and, if they materialized, would not create a just society and that consequently a policy of "convergence" diplomacy, which would seek to produce peaceful change by means of persuasion and closer contacts with South Africa, ought to be pursued.

The study will be hailed as realistic by some and as reactionary by others. The emphasis on international double standards, in the same vein as the South African government, is unfortunate as this will probably lead some to view it as an apology for the status quo. This impression will be reinforced by a number of questionable assertions of which the following are examples: (1) There is "procedural justice in the courts" (p. 5), an assertion which is undermined by the provisions of the Terrorism Act No. 83 of 1967, under which most political offenders are charged (see John Dugard, *Human Rights and the South African Legal Order* [1978] chap. 8); (2) "Defense expenditures do not cause a major political or economic strain" (p. 26), a statement which is difficult to reconcile with repeated government pronouncements that defense expenditures prevent it from spending more on black education and welfare; (3) "Police brutalities are publicly investigated" (p. 34), a statement which requires qualification in the light of repeated refusals on the part of the government to appoint a judicial commission of inquiry into the methods of interrogation employed by the security police.

Moreover, the authors' belief in the lack of corruption in the administration (p. 62) and in the ability of the National Party to change from within have surely been undermined by recent disclosures relating to the Department of Information and by the election of the archconservative Dr. Treurnicht to the post of National Party leader in the Transvaal.

Despite the above comments I prefer to categorize this study as an exercise in realism. The authors are unequivocal in their condemnation of

the National Party's race policies and in their concern for a just society in South Africa. While they advocate closer ties with South Africa as a form of "carrot-strategy" they do not extend such a plea to closer defense contacts on the ground that this would be "politically unrealistic" (p. 70). Their ultimate goal differs little from that of the Progressive Federal Party, which supports a consociational system premised on nondiscrimination and respect for human rights. In essence their disagreement with conventional liberal thinking in the United States relates to the means to be employed to achieve this end.

JOHN DUGARD
Centre for Applied Legal Studies,
University of the Witwatersrand

ASIA AND THE EAST

WOLF MENDEL. *Issues in Japan's China Policy*. New York: Oxford University Press; for Royal Institute of International Affairs. 1978. Pp. xii, 178. \$18.50.

This tightly written, highly readable, and well-researched book offers an interpretation of Japanese attitudes to China over the last thirty years. Wolf Mendl begins with a good summary of the three cycles in Japan's relations with China (1952-58, 1958-65, and 1965-71) and explains how each began with a gradual buildup of relations, only to end in crisis. The author's subsequent analytical framework rests on the four themes that dominated Japan's relations with China for the next two decades: trade, the "two Chinas policy," the problem of security and of the international environment, and the role of the China question in domestic politics.

By 1971 the earlier dream of huge trade with China was dispelled by the economic reality of Japan's global economic interests, in which the China trade was but a small part. At the same time, economic relations with Peking assumed a political importance disproportionate to their economic value.

Japan wanted to accommodate both Chinese governments, but Japan's commitment to Taiwan became increasingly entangled with national security policy. China was no longer a "threat," but the Cultural Revolution and the nuclear tests engendered uncertainty. Throughout, Japan took a very cautious line independent of the United States, forced in part by domestic pressures; there was no domestic consensus to support either Taiwan or Peking. The disagreement did not follow clear organizational lines, that is, neither the dominant Liberal Democratic Party, nor business interests, nor the bureaucracy spoke with one

voice. Each had powerful supporters of Taiwan or Peking. After Nixon's trip to China, and with Premier Tanaka's assumption of the premiership in July 1972, the mood was appropriate for Japan's recognition of China three months later. Taiwan immediately severed relations with Japan. Since then, Japan has studiously avoided any formal declaration of its relationship with Taiwan, yet trade with both China and Taiwan has increased markedly. The China versus Taiwan argument in domestic politics has been replaced by problems of how best to deal with the Soviet Union and China.

Looking to the future, Mendl sees Japan in a web of common and conflicting interests that link Japan, China, and the USSR, with no one set of relationships being wholly hostile, nor wholly co-operative (p. 121). The United States has an interest in maintaining its treaty with Japan as a means of inhibiting any Japanese move closer to the other major powers. China wishes to benefit from Japan's economy, while preventing any penetration of the Chinese economy. China also wishes to prevent Soviet-Japanese collusion and to prevent Japanese domination of South Korea or of the countries in Southeast Asia. Soviet policy also seeks to benefit from Japanese capital and technology and to prevent Sino-Japanese collaboration.

Mendl concludes that "all the major powers wish to use Japan, but simultaneously suspect it. The reverse is also true. Japan is attracted to and suspicious of all the other powers" (p. 121). The United States is attractive for economic and security reasons. China's attraction stems from economic reasons and sentimental attachments, some of which are weakening. Better relations with the USSR are seen to have some economic attractions, but Japanese hostility to the Soviet Union and deep suspicions make them less likely.

The author expects Japan to try to adjust to the complexities of the international environment in East Asia by following a line of studied ambiguity, maximizing options, and trying to reconcile conflicting regional pressures with the demands of its global economic interests.

JAMES H. BUCK
Air War College

ALVIN D. COOX and HILARY CONROY, editors. *China and Japan: Search for Balance since World War I*. Santa Barbara, Calif.: ABC-Clio. 1978. Pp. xxii, 468. \$19.75.

This book consists of an introduction and seventeen chapters, written by twenty-one authors. Chapter one by Chalmers Johnson, "How China and Japan See Each Other," a revision of an article published in *Foreign Affairs* (July 1972), pro-

vides an overview of mutual images. Chapter five is unique in being the only essay on theory and methodology. Chapter six, curiously enough, contains two essays, one on China in Japanese textbooks and the other on Sino-Japanese cultural relations. The remaining fourteen chapters are chronologically arranged and may be divided into three groups: from World War I to the Manchurian Incident, the subsequent period of mounting tension and war, and the post-World War II era.

Two of the four chapters in group one are ably researched case studies on Japanese intervention in Shantung during World War I and on the negligible Chinese Communist Party in Manchuria in the early 1930s. The other two chapters are broader in scope: Shao Hsi-ping's "From the Twenty-one Demands to the Sino-Japanese Military Agreements, 1915-1918" and Chu Pao-chiu's "From the Paris Peace Conference to Manchurian Incident." Nearly half of Shao's article (pp. 38-46) is a repetition of well-known facts. Moreover, Shao herself seems unsure of her subject, remarking that "it is impossible to identify all the [Nishihara] loans" (p. 47). One wishes that she had consulted an excellent work compiled by Suzuki Takeo, *Nishihara shakan shiryō kenkyū* (Research Materials Related to Nishihara Loans [1972]). Chu's chapter also suffers from insufficient use of Japanese sources.

Four chapters in the second group are on peace advocacy. They are informative but are overshadowed by John Hunter Boyle's masterful study *China and Japan at War, 1937-1945* (1972)—including Boyle's own article in this collection. A slightly different version of Winston Kahn's *Doihara Kenji* was published in 1973. These four articles on peace and negotiation are balanced by Alvin D. Coox's work on the magnitude of the Sino-Japanese conflict and its contribution to the victory of the Chinese Communists and by a short essay coauthored by Martin Bagish and Hilary Conroy on two aspects of the question of war guilt, the "rape" of Nanking and the extent of Japanese "imperial" complicity. It is obvious that quantitatively the balance is tipped in favor of peace attempts, and this reviewer thinks that the interpretations are also too "positive" (p. ix). The tendency to downplay conflict is most pronounced in Lin Han-sheng's chapter on appeasers. Here appeasers look more patriotic than nonappeasers. Lin ignores Wang Ching-wei's unpopularity, directly attributable to Wang's pro-Japanese stance, yet criticizes Wang's opponent Chiang Kai-shek for Chiang's failure to "raise the level of consciousness of the people" (p. 236). But, one may ask, to what level did Wang Ching-wei raise the consciousness of the Chinese people?

Lastly, chapters fourteen to seventeen study the

post-World War II era. "Sino-Japanese Relations in the 1970s" has been published before. New contributions are Wang Yu-san's paper on Japan's peace treaty with Taiwan and Yung H. Park's two pieces on Sino-Japanese relations in the 1960s and 1970s.

Several writers in this volume view Sino-Japanese relations in the 1970s with caution: "The long-standing rivalries" between China and Japan "are likely to persist" (p. 16). "Sino-Japanese rapprochement will not advance at a rapid pace in the foreseeable future" (pp. 430-31). The recent signing of the Sino-Japanese treaty of peace and amity and the leap in trade may call for some reappraisal of this conservative estimate. Nevertheless, these articles provide the necessary background for understanding current developments.

The work has an index, although limited in subject listing. The book certainly possesses diversity and enriches our meager literature on China and Japan in the twentieth century.

MADELEINE CHI
Historical Office,
Department of State

CH'Ï-CH'ING HSIAO. *The Military Establishment of the Yuan Dynasty*. (Harvard East Asian Monographs, number 77.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge. 1978. Pp. vii, 314. \$20.00.

The Military Establishment of the Yuan Dynasty by Ch'ï-ch'ing Hsiao consists of a descriptive part with three chapters devoted respectively to discussions of the military system in general, the Imperial Guard (*kesig*) and the garrison system, plus a second part devoted to an extensively annotated translation of chapters ninety-eight and ninety-nine of the *Yüan-shih*, the first two chapters of the *Ping-chih* (Treatise on the Military) section of that work. Hsiao brings a formidable background in both Chinese and Mongolian language to his work, which also is informed by a thorough knowledge of Japanese and other secondary work on the subject. The result is an accurate translation that will stand the test of time and that solves the major translation problems, especially those with Chinese technical terms and Mongolian and other non-Chinese names and technical terms usually encountered in texts of this sort.

The choice of chapters ninety-eight and ninety-nine was dictated by the subject matter of these chapters, which concern military affairs proper rather than other activities, such as horse administration or agricultural colonies, less directly concerned with the military but classified with it by

Chinese bureaucratic habit. The material of these chapters, as indeed of most *chih* sections of Chinese dynastic histories, consists largely of excerpts from decrees, grouped chronologically under a number of broad rubrics. It will not be readily comprehensible to nonspecialist readers but serves instead, as noted by F. W. Cleaves in his foreword, as a basis for further research. The notes to the translation deserve careful attention; they are full and often fascinating expositions of the problems raised in the text.

The descriptive part is of more general interest and should have been more comprehensive. Hsiao deals very well with the subjects he touches and provides definitive answers to a number of questions, such as the precise nature of the Tammaçi army and the role of the *kesig* in the mature Yüan system. I agree strongly with Hsiao's conclusion that the Yüan military system proved inadequate to deal with the rebellions of the 1350s. The analysis, however, becomes progressively weaker as it leaves questions of specific detail and moves on to general historical issues, such as the objectives of the regime in maintaining a military establishment, its success or failure in achieving these objectives, and so on. Most recent work on the Yüan dwells, quite properly, on the transition from the Eurasia-wide Mongolian conquest empire, which held together until the death of Möngke, to the essentially China-centered regime inaugurated by Qubilai. This transition is the main theme in the development of the Yüan military establishment and most other areas of Yüan institutional history. Hsiao is aware of and cites the literature on this subject, which general readers will wish to keep at hand while they read his demanding work.

EDWARD L. DREYER
University of Miami

JONATHAN D. SPENCE. *The Death of Woman Wang*. New York: Viking Press. 1978. Pp. xvii, 169. \$10.95.

This beautifully written and strikingly well-organized book brings together and perceptively presents materials on a subject that until now has had no adequate treatment by scholars in Chinese studies. Jonathan D. Spence's book is about T'an-ch'eng, a small rural area of northeastern China, during the seventeenth century. Relying on three different sources—the local history of T'an-ch'eng county (in Shantung Province); the personal memoir of one of T'an-ch'eng's magistrates; and the essays, short stories, and plays of one of China's greatest short story writers, the Shantung-born P'u Sung-ling—Spence attempts to penetrate a little way into the world of T'an-ch'eng, not only

into the miseries of the area and the poverty of its people but also into the zone of private anger and desperation that were part of the community and the realms of loneliness, sensuality, and dreams that were part of the people's lives.

There are four aspects of life in T'an-ch'eng on which Spence focuses his attention: first, the working of the land and the collecting of land taxes; second, the general social and economic conditions reflected in the treatment of a local widow and her chance to protect her children and her inheritance; third, the ways that misery spawned crime and violence and the effects of raw terror within a rural community; and, fourth, the symbolic fate of a local woman named Wang who was unwilling any longer to face an unacceptable present and chose to run away from her T'an-ch'eng home and husband. These make up the main body, chapters two to five, of the book, which opens with a chapter introducing the sources and closes with an epilogue describing the trial of the murderer of woman Wang.

It should be pointed out that both the title and the dramatic style of the book, plus the omission of the conventional indication of footnotes in the text, might give some readers the impression that it is a nonscholarly work. A close examination, however, of the extensive bibliography and notes at the back of the book, arranged according to page numbers with catchword entries, reveals the author's thorough and conscientious effort in aiming at a high standard of scholarship. By skillfully interweaving his sources, the author draws a bleak and soul-stirring picture of rural desperations that lead our attention deep into the tragic world of T'an-ch'eng, where people suffered helplessly through natural disasters, internal uprisings, and foreign invasion. T'an-ch'eng suffered passively, paid its taxes, yet received little in return. By the time the *Local History of T'an-ch'eng* was compiled in the early 1670s, a quarter of a century after the Manchu conquest of China, the population of T'an-ch'eng was only about one-fourth of what it had been in the later Ming dynasty fifty years before, and the area of cultivated land registered for taxation had dropped by almost two-thirds (p. 3). Furthermore, for the sixty years between 1646 and 1708, not a single student from T'an-ch'eng passed the second level *chü-en* examination (p. 17); with such a poor showing in the civil service examination, it is understandable that T'an-ch'eng had lost out in all the observable distributions of influence, power, and wealth.

The book ends with woman Wang, for it is through her desperation that the author captures the rural misery of T'an-ch'eng. The story of woman Wang was originally recorded in the personal memoir of a one-term magistrate (1670-72)

who was reminiscing about his own experiences in T'an-ch'eng. More interested in the murder case and its subsequent trial than in the victim herself, the memoir gave very little information about the woman and her husband. It merely related the bare facts that the husband was an ill-provided hired laborer surnamed Jen, that they married sometime in the late 1660s, that they were very poor, that the woman had bound feet and was left alone much of the day, that they had no children, that in early 1671 she ran away with another man but after a short time was abandoned by her lover who left her alone on the road, and that finally in desperation she returned to her husband and died at his hand.

Perhaps it is the contrast between the available details about the environment of T'an-ch'eng and the skimpy information about woman Wang that makes her stand out as a representative of the mass of the poor and the forgotten. Viewing her as a representative of all the miseries and sufferings of her rural community, Spence attempts to penetrate into the subliminal consciousness of woman Wang through the written and collected memory of Shantung. It is at this point that the Shantung-born P'u Sung-ling enters. Having drawn on P'u in three dimensions—as a recorder of Shantung memories, as a teller of tales, and as a mold of images—Spence uses P'u's angle of vision to supplement the more conventional historical writings. For example, he combines many of P'u's literary images in montage form to describe "what might have been in the mind of woman Wang as she slept before death" (p. xv). The effect is powerful and tremendously moving. Here Spence shows not only his ingenuity in writing, but his deep-felt compassion and sensibility toward humanity.

If there is any area on which one might cast a critical eye, it would be in the area of women's studies. In a book where one woman is symbolic of misery, a general analysis of women's roles and conditions in their society seems imperative. Without such an analysis, attempts to examine any isolated problem of the woman will be somewhat unsatisfactory. This probably explains why the third chapter, "The Widow," which focuses on the widow's problems in her attempt to protect her children and her inheritance, is the weakest and least informative among the five chapters of the book. It is true that in the area of women's studies, paucity of primary sources remains a chief problem: the biographic section of "Honorable and Virtuous Women" in both local histories and Standard Histories are still the main source. But as interest in women's studies grows, useful topical studies of Chinese women are available; and there are new sources of materials being explored, the most promising among them being the mine of

popular literature. No doubt Spence is fully aware of the limitations of the conventional sources; his constant supplementing of them with works of literature is self-evident. One would only suggest that a more extensive discussion of the place of women in the early Ch'ing society might prove useful and beneficial.

Spence's book breaks new ground for Chinese historical studies. It opens new visions of traditional China heretofore unexplored in the current scholarship on Chinese history. It is a major achievement of historical research in many ways—in its skillful use of sources, in its dramatic presentation of materials, in its perceptive interpretation, and, most important of all, in its successful attempt to make far-off rural China a part of our own experience and the stories of woman Wang and her fellow countrymen a part of our vocabulary.

SHELLEY HSUEH-LUN CHANG and
CHUN-SHU CHANG
*University of Michigan,
Ann Arbor*

A. S. KOSTIAEVA. *Krest'ianskie soiuzy v Kitae (20-e gody XX veka)* [Peasant Unions in China (1920s)]. Moscow: Izdatel'stvo "Nauka." 1978. Pp. 186. 1 r. 60 k.

The bold spirits who ventured out into the South China countryside in the early 1920s to organize the peasantry for revolution had little to guide them but an intuitive sense of the peasantry's potential power, and at first their efforts were meagerly rewarded. Yet within five years peasant unions or associations had spread northward so rapidly that the youthful Mao Tse-tung entertained visions of an imminent revolutionary tidal wave in the Chinese countryside that would sweep away the old order. A. S. Kostiaeva's valuable monograph, one of a growing number of Soviet studies of Chinese agrarian problems in the 1920s, is the first to zero in on the development, composition, activity, and significance of the peasant unions themselves. This is both its strength and weakness.

Kostiaeva rejects the standard Soviet depiction of the peasant unions as modern organizations of the rural poor that represented the vanguard of the rural revolution. Stressing the great diversity in their types of organization, social composition, activity, standing in rural society, and so forth, she indicates that the peasant unions were generally far less influential or powerful than is often supposed. In many ways peasant unions bore a greater kinship to traditional groups such as secret societies or self-defense corps than to modern revolutionary organizations. The economic demands

put forth by peasant unions were more often for a restoration of older notions of economic justice than for land redistribution or confiscation. (Her argument here is similar to that of James Scott, *The Moral Economy of the Peasant* [1976].)

Kostiaeva also points to the crucial connection between Kuomintang military power and the spread of peasant unions, which proliferated in the wake of the National Revolutionary Army's successful Northern Expedition in 1926. But, anxious to maintain order behind their lines, Chiang Kai-shek's armies (supported by Communists within the united front) usually discouraged expressions of spontaneous peasant radicalism. The bitterest enemies of the peasant unions were local interests—landlords, gentry, rural mafioso, and so forth—whose organized opposition and superior violence ultimately crushed these early experiments in Chinese rural organizing.

A more comprehensive and balanced assessment of the peasant unions is possible only by analyzing them in the context of the overall problems of revolutionary strategy and the debates going on within the KMT-CCP revolutionary leadership in the 1920s. Here Kostiaeva is far less helpful as a guide. A graver defect for a monograph is the author's failure to convey very much of a sense of the political dynamics of the movement and a feeling for the particularities of time and place or of the historical actors in the peasant movement. Fortunately, one may turn to another recent study on roughly the same subject, Roy Hofheinz's *The Broken Wave* (1977), which links a sophisticated discussion of problems of rural revolutionary strategy with fascinating case studies of the peasant movement in several specific rural counties. It is noteworthy that Hofheinz and Kostiaeva broadly agree in their evaluations of the peasant unions, and, taken together, go far to advance our understanding of a fascinating problem in modern Chinese history.

STEVEN LEVINE
Columbia University

ARIF DIRLIK. *Revolution and History: The Origins of Marxist Historiography in China, 1919-1937*. Berkeley and Los Angeles: University of California Press. 1978. Pp. x, 299. \$17.50.

Arif Dirlik has produced a valuable study of Marxist historiography. Indeed, the subtitle of the book is unduly modest. Although it describes accurately enough what is found in the central four chapters, it fails to convey the fact that the study succeeds also in demonstrating the vulnerability of Marxist thought when it is applied outside the West, when it is only partially unfolded, and when it does not enjoy a fair degree of freedom.

The story of the appeal of historical materialism to Chinese revolutionaries has been told before, but the context here is fuller and the questioning more multifaceted than in earlier writings. And this has enabled the account of the "social history controversy" of 1929–33 to come alive in unexpected ways. The two best-known men involved in this controversy, T'ao Hsi-sheng (now in Taiwan) and Kuo Mo-jo (who died in Peking in 1978), are given enough attention, and their several adaptations of Marxist ideas for the writing of Chinese history come across clearly. Other protagonists also get a fair hearing, but the reader would need some patience with several of the permutations and combinations that they managed to produce with what they each thought were the correct stages of historical change.

Despite the obvious distortions and obscurities arising from enthusiasts having been carried away by heated debate—we are grateful to Dirlik, who makes several essays seem more lucid than they are in the original—the main difficulties encountered with concepts like slavery, feudalism, and the Asiatic mode of production and with the periodization of history are clearly examined and placed in context. In particular, the writings reveal the variety of minds at work and the wealth of ideas being tried out. Dirlik succeeds in showing us that not only heat but also a fair amount of light was generated in these few brief years.

The book suggests in the end that Marxist historiography might have borne more fruit under anti-Marxist governments than under the Marxist-Leninist government of the People's Republic of China. Indeed, the main outlines of the new historiography seem to have taken shape during the exciting years 1929–33; much of the work done since 1949 seems to have been obstructed by the "Stalinist" orthodoxy that had been merely one of several strands in the 1930s. Dirlik is very persuasive and goes a long way toward explaining why Marxist historiography became so deadly in a socialist society that lacks the background of pluralism and the vigorous and far-ranging debates of the 1930s.

It might, of course, also have been that historical materialism provided just about the most exciting set of new ideas available to Chinese intellectuals in the 1920s and 1930s and that period cannot be duplicated. Since 1949, it has become the hard slog of rewriting Chinese history to fit the new frame. Dirlik, however, encourages the reader to believe that all may still be well if the Chinese can turn around and insist on the complexity of their long history and, for a start, abandon the unilinear features of Marxist theory.

WANG GUNGWU
Australian National University

G. B. ENDACOTT. *Hong Kong Eclipse*. Edited and with additional material by ALAN BIRCH. New York: Oxford University Press. 1978. Pp. xiv, 428. \$21.50.

At the time of his death in 1971, George B. Endacott, senior lecturer in history at the University of Hong Kong, had completed this work on the colony's wartime experience. About one-third of the book is devoted to prewar defense preparations and the Japanese invasion. The occupation, including experiences of internees and prisoners of war, fills a second portion. The remainder deals with liberation and the subsequent transformation of the colony. The book's perspective is unabashedly colonial: Endacott's "history from the top" emphasizes legal and administrative forms and makes only a minor effort to fit the colonial experience into the perspective of imperial concerns. For example, the debates about retrocession of Hong Kong at the end of World War II, explored by William Roger Louis in *Imperialism at Bay* (1977), are only touched upon by Endacott.

Since the book was not intended for scholars, Endacott used few footnotes, a deficiency partly remedied by Alan Birch, Endacott's literary executor, who added a bibliography and appended several essays to the text. Much of Endacott's material came from personal accounts. He wrote prior to the opening of British archives and did not use Chinese or Japanese sources, although Birch consulted Chinese and some translated Japanese material. Consequently, the book focuses on Hong Kong's British elite and largely ignores the relations between the British and other ethnic communities. The work reflects, in sources and perspective, the distance of the British governing class from the Chinese, and it echoes the relatively unsympathetic view of the nationalists held by Whitehall.

Endacott's stated theme is the Japanese conquest and occupation of the colony. The campaign was rapid and significant only as a blow to British prestige. Endacott does not explore the governmental policy that led to the futile defense of Hong Kong. Readers who desire to understand the failure of planning in Britain's inadequate preparations for war in East Asia can consult Raymond Callahan's *The Worst Disaster: The Fall of Singapore* (1977); *Hong Kong Eclipse* examines the results of that policy only for the local situation. According to Endacott, the Japanese occupation had no lasting influence. Due in part to the pressure of the war, the Japanese produced no constructive government or reforms in the colony's social systems. Furthermore, Japanese ideological influence was swamped in the massive postwar immigration from mainland China.

The book's real theme, however, best captured

in the chapter on life in Hong Kong under the Japanese and in Alan Birch's appended essay on the social and political impact of the war, is the wartime transformation of the colony. On the eve of World War II, Hong Kong was, in Agnes Smedley's words, "like a rotten fruit." That there was no revolutionary upheaval can be partly attributed to the deplorable conditions in surrounding Kwangtung. Japanese conquest brought looting, but Japanese imperialism meant removal of British dominance, not revolution. The Chinese population, intent on survival, remained largely unengaged by either British or Japanese blandishments.

Yet a revolution did occur. Its demography is stark enough. In June 1941, the population of Hong Kong was about 1,700,000; in August 1945, there were fewer than 600,000 people in the colony. In the midst of this exodus, the Japanese paved the way for a new pattern of oligarchy. After the war, most of the British who had been interned departed for good, to be replaced by a new group that was unable or unwilling to sustain the old colonial arrogance. The massive influx of Chinese who sought security from civil war furthered the dissolution of the old Hong Kong. The process was completed by the rise of the People's Republic of China and the resultant closure of the border, which created a new sense of permanence in the minds of Chinese residents.

CLARENCE B. DAVIS
College of Charleston

SUSAN B. HANLEY and KOZO YAMAMURA. *Economic and Demographic Change in Preindustrial Japan, 1600-1868*. Princeton: Princeton University Press. 1977. Pp. xiii, 409. Cloth \$27.50, paper \$10.75.

The reason for this book is to offer an alternative to the "stagnation" doctrine prevailing among Japanese Marxist historians of the Tokugawa period. Back of the Susan B. Hanley-Kozo Yamamura revisionist interpretation is a growing awareness of a paradox, a steadily growing economy but widespread practice of population control. Even the Marxist historians accept that economic growth occurred but contend that increased wealth accrued only to the ruling elite and rich peasants, not to the rural and urban masses. The contrary conclusion reached by Hanley and Yamamura is that the exploited masses participated broadly in a rising material abundance and that indeed, the non-urban population's gain was relatively more than that of the townspeople. In time, the masses were generally better off than the samurai living on fixed stipends and burdened with heavy financial demands of status. The essential article of the

Marxist dialectic, "the impoverishment of the villages," is turned on its head.

If the authors' argument is not altogether proven, the scope and weight of evidence is persuasive that the Tokugawa economy was growing comfortably and at a higher rate than population, resulting in a generally rising living standard. Propounding this radical view entails an ambitious program of analysis, however, since the authors' argument rests on the very data by which Japanese Marxist scholars come to just the opposite conclusion.

Irrespective of widely varying local conditions and despite repressive constraints on private enterprise and ostensibly rigid controls on labor and taxation, living conditions improved palpably such that parallels between preindustrial Japan and Western Europe invite comparison. Similarity of preconditions of growth in both cases suggest common demographic strategies. Popular attitudes favorable to the demographic strategy of containing population growth below the carrying capacity of slowly but steadily expanding productivity were critical in raising expectations of quality of life.

The authors' lengthy investigation of Tokugawa economics and demography allows them to integrate a broad range of rich but geographically and qualitatively uneven materials. In the larger sense, the book is an assessment of work to date in the critical reexamination, begun in the 1950s, of standard assumptions about Tokugawa demographic patterns and their relationships to economic trends. Alongside indicators of increased output of the economy as a whole, there are good grounds for believing that the rural sector, ordinary farmers and nonurban merchants, made the highest per capita income gain and enjoyed substantially the highest rate of capital formation in the country. Not only did polder reclamation expand at an exponential rate, but nonfarm production sectors were also developing dramatically. Establishment of a unified trade network extended commerce universally in spite of *bakuhau* authorities' strenuous efforts to restrict and control it.

This bullish picture of a dynamic, growing economy finds no more eloquent support than the contention that, although during the seventeenth century population increase was higher than in the rest of the period, productivity increase rates between the two subperiods tended to be just the opposite. An increasingly mobile labor force and a strong shift to an open labor market drew peasants steadily out of agriculture to manufacturing and commerce, a tendency that attained crisis proportions by late Tokugawa. Meanwhile, birth rates seem to have been falling, while mortality remained fairly stable. The shortage of labor, incurred by conscious limitation of family size, plus

greatly eased labor mobility, caused an irresistible urge to improve living standards and to minimize the number of nonworking family dependents, thereby insuring the greatest return on each worker's labor. The Tokugawa strategy of population control after the seventeenth century arose not out of poverty but rising expectations of income and quality of life.

The authors' basic approach to these uneven and discontinuous data is to deal with parameters longitudinally by a best-case/worst-case regional comparison. Thus, the most advanced Kinai and Okayama-han and the least advanced Tohoku and Morioka-han represent the regional economic polarities. Sampling and comparing the areas of highest growth with those of lowest permits checking economic trends against those of population, for example, labor supply against labor costs, domestic consumption (which rose inexorably), taxation and rent revenue flows, and estimated productivity.

Contrary to the conventional view, in both regions living standards grew, though at different rates and with different mixes of the same factors. For example, indexes of wage levels and rice consumption argue plausibly that living standards in Morioka (the poorest of the poor) were higher than has been assumed. The essential point of this analysis, supplemented by a close-grained study of one domain and a village case study, is the consistency with which the evidence points to just the opposite of the ideologically flavored established interpretation.

Looked at on this scale and through this framework of economic analysis, the doctrine of deprivation of the peasantry and nonurban business classes by the Tokugawa system is badly shaken. Although unfinished in this volume, the argument for trade-off of numbers for greater average consumption in the latter Tokugawa seems unassailable.

JOHN B. CORNELL
University of Texas,
Austin

PAUL WHEATLEY and THOMAS SEE. *From Court to Capital: A Tentative Interpretation of the Origins of the Japanese Urban Tradition*. Chicago: University of Chicago Press. 1978. Pp. xii, 242. \$15.00.

In this brief but provocative work the authors enter, without necessarily trying to win, virtually all the debates of early Japanese history, including the nature of the Wa polity, the location of Yamatai, the probability of a conquest dynasty from the peninsula, and the origins of the *uji*. They offer the book not as a definitive statement but as an

"extended conjecture," subject to revision by new evidence.

The study is an outgrowth of Paul Wheatley's *The Pivot of the Four Quarters: A Preliminary Inquiry into the Origins and Character of the Ancient Chinese City* (1971). He has added new theoretical material to his approach, and, in cooperation with Thomas See, has turned his attention to neighboring Japan as a case of "secondary urban generation," in effect describing how Japan came to adopt the Chinese urban model for its capital cities.

Although it deals with Japan's urban origins, the book is also an examination of the process of state formation, since the authors argue persuasively in their introductory chapter that the "generated city in its early phases was a city-state, with the clear implication that city and state were coeval and, indeed, constituted different-scale manifestations of the same process" (p. 8).

The book is valuable from several standpoints. The skillful application of theoretical models from various social sciences helps explain the developments of early Japan more systematically. It is to be hoped that Japanese and Western scholars of Japan will be encouraged to delve more deeply into their sister disciplines for useful analytical tools. The authors generally achieve a harmonious union of history and social science, although at some points a heavy usage of jargon tends to confuse rather than clarify an argument. They further introduce to a wider audience the works of important scholars of early Japan, notably Mizuno Yū, Egami Namio, and Gari Ledyard. And, although Wheatley and See rarely accept *in toto* the ideas of any one scholar on a specific issue, they take what they consider the most reasonable parts of often diametrically opposed theories and weave them into a very credible account of Japan's early development.

The authors are dealing with an extensive subject and an enormous body of materials in a brief space. Thus, although the reader is frequently stimulated, and occasionally provoked, by their approach, he is at the same time frustrated that they did not bring their considerable analytical talents to bear more thoroughly on some of the many problems they have sketched. Nevertheless, this book is a tremendous contribution to the growing body of Western scholarship on ancient Japan, and it should prove highly suggestive to the comparativist as well.

G. CAMERON HURST III
University of Kansas

ROBERT J. SMITH. *Kurusu: The Price of Progress in a Japanese Village, 1951-1975*. Stanford, Calif.: Stanford University Press. 1978. Pp. xvi, 269. \$15.00.

Kurusu is a superb history of the tremendous changes that have taken place in rural life in postwar Japan. Written by an anthropologist with a good sense of history, it deals with the alterations that have occurred in every aspect of what is known as "village Japan"—the composition of the population, changes in the economy (including the decline of agriculture), changes in the family and living arrangements, and the revolution in the standard of living—and their effect on community and interpersonal relations. This book is worthy of review in a historical journal because Robert J. Smith has interwoven the events in *Kurusu*, a hamlet comprising a score of families, with changes at the national level. Thus, in a real sense, we have a history of "the" village in postwar Japan, rather than merely of "a" village. So well done is this study that, despite my long involvement with Japan, I was only fully made aware of the postwar revolution in life styles after reading it.

Smith's subtitle, *The Price of Progress in a Japanese Village*, refers to the loss of community and harmony the hamlet has experienced as a result of participation in a wider world—the decline of the village as the major source of income and the use of machines, both farm and domestic, for activities that used to be undertaken cooperatively. The specific catalyst for this loss in *Kurusu* was the "Clover affair" of 1975, in which the hamlet split over the question of whether a chicken processor should be permitted to set up a factory within the village. The opponents of the venture, those fearing the pollution it might cause, successfully blocked it, but the rift was such that the community as a whole feels that the harm done to interpersonal and community relations will never be repaired. The damage, however, was not caused by the disagreement between residents *per se*, but by the fact that it was carried out on a public level, with newspaper and television coverage, and not in the privacy of the *jichikai*, the self-governing association.

It is only the sections on the decline of hamlet solidarity and on the generation gap (chaps. 6–8) that leave the reader with unanswered questions. What are community relations like in the vast majority of villages that have not experienced this unusual rift in community relations? And Smith's treatment of the generation gap leaves the reader with the feeling that he would like to see a psychologist study how these people really feel about each other. All of this is, of course, necessarily beyond the scope of Smith's book, and it is the mark of the true scholar that he can point to directions for future research.

Comparisons between spending patterns, wages, and so forth would be easier to grasp had the

author included an index of changes in real and nominal income, instead of merely the dollar-yen exchange rate. This book, however, is extremely well-written and Smith always takes pains to present his work so that the nonspecialist can fully understand it. I cannot recommend any book more highly.

SUSAN B. HANLEY
University of Washington,
Seattle

GORDON MARK BERGER. *Parties Out of Power in Japan, 1931–1941*. Princeton: Princeton University Press. 1977. Pp. xv, 413. \$20.00.

One of the recurring questions in modern Japanese history concerns the significance of the 1930s. Do they represent an aberration, a dark valley between the bright peaks of "Taisho Democracy" in the twenties and the "miraculous recovery" of the postwar era? Do they constitute the culmination of an inevitable trend toward fascism brought about by the superimposition of capitalism upon a still feudal society in the nineteenth century? Or did military dominance of the Japanese political scene in the thirties simply stand for a "normal" development in a land with a long tradition of military rule?

In this detailed work on Japanese elite politics in the 1930s, Gordon Mark Berger provides what has come to be expected of American historians of Japan: a well-researched, nonideological, and somewhat tedious study that fails to address the above questions. To say this, however, is not to criticize the book, which is a valuable contribution to the English-language literature on modern Japan. Most previous studies by Americans have approached the period either in order to explain the background to war or to examine the prewar activities of the postwar left-wing parties. Departing from this to concentrate on the bourgeois political parties, Berger shows that these organizations were virtually indestructible. Although all political parties in Japan were officially dissolved in the summer of 1940, they retained their parliamentary power, their local bases of support, and their leadership structure, to re-emerge after 1945 with new names to dominate national politics. Thus, as Berger implies, the political transformation of postwar Japan was not as dramatic and surprising as often supposed. What is amazing is the continuity of Japanese institutions, a fact that is brought to the reader's attention.

After a brief introduction on the history of Japan's bourgeois political parties up to 1930, Berger devotes two chapters to the decline of party authority and the concomitant rise of military influ-

ence up to the 1937 designation of Konoe Fumimaro as premier. This part of the book contains little that is new, but is a necessary prelude to the remainder, which consists of three immensely detailed chapters on the late thirties. Drawing mainly on memoirs, secondary accounts, and some interviews, Berger provides a blow-by-blow account of the political struggles of the various elites of which the parties were but one. Occasionally this section gets bogged down in minutiae, but it is also the most valuable part of the book. A high point is the author's keen description of the repeated attempts to mobilize the masses of Japan through various devices including the Imperial Rule Assistance Association in late 1940.

On the whole, this work is worthy of more praise than criticism. It does not replace the many excellent works on the period in Japanese, but is the kind of "solid" treatment that may become standard. I fear the readership will not be large, but copies of it should stand on the shelves of university libraries for many years to come.

DANIEL B. RAMSDELL
Central Washington University

RISHIKESH SHAHA. *Nepali Politics: Retrospect and Prospect*. 2d ed., rev. New York: Oxford University Press. 1978. Pp. xii, 291. \$11.50.

NARI RUSTOMJI. *Bhutan: The Dragon Kingdom in Crisis*. New York: Oxford University Press. 1978. Pp. 150. \$8.95.

In two of the smallest and most remote states in the world the beckoning trails to modernity are filled with perils. Both monarchies with ancient ways still strongly in place, Nepal and Bhutan relentlessly trudge ahead on the courses charted by others with, as it were, joyless expressions on their faces. They know well what they are leaving behind but have no clear vision of what lies ahead. Switzerland of Asia? Appendages of India's chaotic industrializing culture? Miniature show-cases of village-level democracy? Rishikesh Shaha, the Nepali patriot and scholar, and Nari Rustomji, the sympathetic, hard-working Indian advisor to Bhutan, give no answers. But their well-produced books raise every conceivable question.

Shaha's *Nepali Politics* adds impressively to the finely structured analyses of Nepal's domestic and foreign policies published in the past two decades. Shaha is a master of American modernization theory and enriches it with the intimate knowledge of the insider. His bibliography, economic data charts, and appended documents alone make the work an essential addition to basic libraries on Nepal and modernization. In four separately writ-

ten essays (chapters) on national integration, *panchayat* politics, foreign policy, and (for this edition) the working of the 1975 constitution and recent foreign policy, the author explores all practical and philosophical problems of Nepal's public life. Two examples may convey some of the substance: Stronger administration, Shaha finds, creates an "imbalance between the administrative and political bases of power" because "leaders are prone to exploit the administrative services for political purposes" (p. 31). "Political participation" in Nepal thus may not be the path to democracy but to bureaucratic feudalism. To remain independent, Nepal pursues in foreign policy a classic buffer state strategy whose success "depends on its ability to assess . . . what India and China regard as their minimum interests in a particular matter at a given time and, on the basis of this assessment, to strive for concessions and gains from all parties" (p. 185).

Bhutan's politics, devoid of the blessings of institutionalization, center around the royal family, the Wangchuks, and, during Rustomji's tenure as Indian advisor (1963-66), the Dorjis. A genealogical chart (unfortunately lacking dates) traces the extraordinary linkages of these two powerful clans and the related Namgyals of Sikkim. Modernization, partly embodied in Rustomji's development activities, inevitably hurled against one another old time army officers and lamas on the one hand—not distinctly characterized perhaps because everything important in Bhutan is secret—and on the other hand the Westernized Dorjis who innocently offended people by introducing roads, foreign experts, and visitors like Shirley MacLaine into the Dragon Kingdom. Jigme Palden Dorji, to whom the book is dedicated, paid with his life in 1964. His assassination, never conclusively adjudicated according to the author, and the tragic repercussions that followed are the subjects of Rustomji's swiftly moving, dramatic account. King Wangchuk, married to Jigme Dorji's sister but also suspicious of the rival Dorji family, emerges as a pathetic figure seeking solace in his mistress and in frequent foreign visits for his heart trouble. Politics seem primitive compared to the Nepali arena. Is Bhutan headed for an Iranian type of modernization, Sikkim-style absorption into India, or something else? This book merely sets the stage for subsequent answers, with a mystery story as a brilliant opener.

CHARLES H. HEIMSATH
American University

R. D. HILL. *Rice in Malaya: A Study in Historical Geography*. New York: Oxford University Press. 1977. Pp. xvii, 234. \$39.50.

No comprehension of Southeast Asia is possible without an understanding of rice cultivation: its origins and diffusion, its major types, and their economic, sociopolitical, and also cultural implications. For some countries in the region an adequate literature exists but not for Malaya. Short accounts, descriptions, official reports, and statistics are relatively plentiful, but an encyclopedic survey and even many of the necessary ingredients for one have yet to appear. By addressing this need R. D. Hill's book accordingly makes a welcome contribution, but—disappointingly—a narrower one than its invitingly broad title and subtitle suggest.

In scope and method, this is a limited study. Three preliminary chapters sketch the prehistory and precolonial history of Malayan riziculture. Five principal chapters cover the country's major agro-economic zones from the early nineteenth century until (somewhat arbitrarily) about 1910, when an overall colonial agricultural policy began to be devised. A final chapter makes explicit the typology of increasingly intensive agricultural regimes that informs the preceding discussion. Hill's method, meanwhile, is essentially to trace, excerpt, condense, collate, and comment upon scattered descriptions, recorded mainly by foreign observers, of the various agricultural districts. Although, with some exceptions, his use of published, though not archival, sources is impressively thorough, this method is fundamentally flawed. As the author partially recognizes, his account is inevitably imbalanced, partly because the less important rice-growing areas were best known to Europeans and thus most often described by them, while the generally more remote regions, where intensive rice cultivation dominated, long remained largely unreported. The account is also unbalanced because, reflecting the selective vision of his sources, Hill in effect offers a historical geography not of Malayan rice cultivation but only of agricultural land settlement and land use. Relatively neglected, in consequence, are the cultural ecology, the social and legal relations, and the political economy of peasant cultivation. The seriousness of these omissions is all the more marked since Hill's analysis is at its best precisely when (as in his discussion of the conflict between sugar and rice in the Krian irrigation area) his sources permit him briefly to address such matters. It is further implied by Hill's main conclusion: that an increasingly exclusive peasant reliance upon intensive rice cultivation is not ancient practice but a relatively recent phenomenon, or process, resulting from the impact of colonial policy and the world economy.

Nevertheless, together with Lim Teck Ghee's recent *Peasants and their Agricultural Economy in Colo-*

nial Malaya, 1874-1941 (1978), to which it stands, in effect, as a prologue, Hill's book does render a valuable service: it provides some foundation for the inclusive study of the history of rice in Malaya and of the manifold and changing role of rice in Malay history and society, that has yet to be written.

CLIVE S. KESSLER
Barnard College

A. W. STARGARDT. *Australia's Asian Policies: The History of a Debate, 1839-1972*. Hamburg: Institute of Asian Affairs in Hamburg, and Otto Harrassowitz, Wiesbaden. 1977. Pp. xxii, 404.

The last century transformed Australia from a Pacific area into an outlying region of Asia. A. W. Stargardt explores the record of Australian relations with Asian states and perceives it as an extended debate between conservative and radical Australian policies focused upon the threats of invasion by alien ethnic and cultural forces. This expectation was encouraged by Australia's isolated location, small population, and vulnerable coastline.

Before the 1860s Britain used Australia in connection with a forward China policy. Subsequently, the self-governing Australian colonies groped toward neutralism, but this radical impulse did not prevent conservative leaders from sending Australian contingents to the Sudan, to South Africa, and to China to join the Boxer Expedition. During World War I, the Australian Commonwealth loyally supported the empire, but its leaders were deeply concerned by Britain's concession of Pacific islands north of the equator to Japan. An Australian Navy was established in this era, and RAN vessels supported British operations in China during the twenties, although the Labor Party pressed for an Australian policy.

No distinct foreign policy, independent of Britain's, appeared until just before World War II, when embassies were established in Washington, Tokyo, and Chungking. But Prime Minister R. G. Menzies announced that no declaration of war against Germany was necessary since a British declaration automatically carried Australia into the conflict. This point of view lost ground when the Labor government of John Curtin took charge in 1941. Australia's declaration of war against Japan was, Stargardt states, the first instance in which an Australian government had acted as a sovereign state in international affairs (pp. 175-76). Subsequently, the traditional reliance upon Britain was set aside and combined military arrangements made with the United States. The Australian divisions that Menzies had sent to fight

Britain's battles in the Middle East were brought home by Curtin, despite Churchill's protests, to defend Australia from Japanese attack.

As the war ended, the Labor government played a part in postwar arrangements and gave support to emerging Asian states but retained the traditional suspicion of Japan. In 1949 the conservative Liberal-Country coalition returned to power for twenty-three years and reverted to the prewar policy of dependence, with the United States taking the place of Britain. The ANZUS Pact became a central tenet of Australian policy. This policy, Stargardt feels, led inevitably to SEATO, Korea, Malaya (1965), and Vietnam, and he applauds E. G. Whitlam's proposals for a Labor Party program more responsive to Asian outlooks and needs.

One wishes that the author did not moralize quite so much; the evidence ought to speak for itself. It is difficult to justify the inclusion of sketches of Southeast Asia during World War II (pp. 183-202) that contain only a few brief references to Australia. Several powerful influences upon public views of Asia in nineteenth- and twentieth-century Australia, such as the Australian Natives' Association, and the writings of William Lane, the radical, racist editor of the Brisbane *Boomerang*, are not mentioned. Neither is the Gallipoli campaign, where Australians first met Asians in mass warfare, nor the Colombo Plan (1951), which still offers technical assistance to Asian nations, a plan cosponsored by Australia and Ceylon. Finally, the editing is, at best, slapdash. Footnote twenty (p. 126) is missing in the endnotes (pp. 372-73), indexing is inaccurate, and there is no bibliography.

CHARLES S. BLACKTON
Colgate University

UNITED STATES

ROBERT F. BERKHOFER, JR. *The White Man's Indian: Images of the American Indian from Columbus to the Present*. New York: Alfred A. Knopf. 1978. Pp. xvii, 261. \$15.00.

The focus of this book is upon the "images" of "The Indian" held by the whites who found him in America. Taken as a whole, the book is learned and well informed, abbreviated in coverage, and quietly ideological.

At the very outset, the author makes a strong and self-conscious case for his conviction that continuity (rather than change) best describes white American images about the Indian. At times, he almost seems to say that nothing has changed

since the very beginning of first contact. His treatment of the subject is far too wise and subtle for him ever to sustain such a proposition, but this assumption governs the organization of the book.

The study begins with a description of the reaction of the first European migrants to the people they found in the New World. His emphasis appears to be that the Spanish, French, and English reactions to the aboriginals of the Americas were more alike than different; a similar emphasis reappears in a later chapter. Given this stress on continuity, it is not surprising that the ensuing parts should chop up "images" into the following categories, in each case spanning more than a century. "Part Two, From Religion to Anthropology, The Genealogy of the Scientific Image of the Indian"; "Part Three, Imagery in Literature, Art, and Philosophy"; "Part Four, Imagery and White Policy, The Indian as Justification and Rationale." Such an organizational framework has benefits, but it also has costs. Although there are cross references among the parts, interconnected developments are treated separately: scientific, literary, and public policy concerns about Indians, for example, might well be better examined holistically.

A steady flow of sensitivity and just plain sensibleness pervades the study. The footnotes at the end constitute a first-rate bibliography on the subject. There are eleven well-chosen illustrations accompanied by highly illuminating commentary. The writing is clear, though far from elegant.

The most essential theme of the study is that white Americans in the United States tended, from the beginning, to homogenize widely various Indian tribal groups into "The Indian." Various images were attached to this mental creation, ranging from the noble and innocent savage to the bloodthirsty and vengeful warrior. In every instance, of course, the image served the perceptual and practical needs of white Americans. The author concludes by pointing to the irony that in recent years American Indians have adopted a similar homogenized view of themselves for their own political and cultural purposes. Thus the original white conception of reality has been turned around by the victims.

There is, however, a hidden agenda underlying this book. The author has thoroughly committed himself to an ideological posture concerning cultural relativism. He would like to see the Amerindian nations, as well as the dominant invading culture, assessed on their own terms, by their own standards. Further, he would like to have our own current understanding of this past experience measured by the same yardstick of culture-boundness. Both intellectual and moral judgments are involved. If, on the one hand, one grants various Indian groups total cultural legitimacy no matter

what they think and do, then one must make the same concession to the English and other settler/invasers. The same principle applies to modern understanding of the experience involved. At one level, such an intellectual and moral position is unassailable; at another, it is a total shambles. One almost wonders whether Lewis Carroll's Red and White Queens came from a chessboard in North America.

The whole subject is far too complex for treatment in two hundred pages. (It took Roy Harvey Pearce nearly as many words to discuss the literary evidence about images of Indians prior to the Civil War.) In my opinion, "Cultural Anthropology and the Modern Conception of the Indian" is a book-length subject. Here it receives seven pages, and there is startling foreshortening. We have here a brisk discussion of Boas, Kroeber, Kluckhohn, "English social anthropology," Underhill, and a block quotation from Eleanor Leacock, who is surely less than centric to the field of modern anthropology. In fact, current anthropology, taken as a whole, has scarcely adopted the posture of "agnosticism and relativity" which the author ascribes to it; nor has it adopted a posture which the author says "certainly represents an expression of alienation" toward "One's own society" (p. 68). Such a proposition suggests that any book is merely a cultural artifact of its times. True for sure, but false for certain.

WINTHIROP D. JORDAN
*University of California,
Berkeley*

FREDERICK MERK. *History of the Westward Movement*. New York: Alfred A. Knopf. 1978. Pp. xvii, 660. \$20.00.

This is a lengthy, sometimes overly detailed account of the development of an area that became the forty-eight United States. Chronologically it extends from the pre-Columbian period to the mid-1970s, or shortly before the author died (1977) at the age of 90.

The reviewer is aware that for years this journal warned these assessing books for it that they must not criticize an author for not having written another book. While there is no desire to do such a thing here, it is difficult to describe the work at hand without some reference to what it is not. This is especially true when considering the title, for titles can be misleading to potential users and, to a certain extent, that is the case here.

Although the book contains chapter titles bearing the words "advance" and "settlement," there is very little description of the westward movement of people and institutions. Transportation, a key to

the westward movement, is much neglected. On the other hand there is much about sections and sectionalism, about slavery as it influenced the sectional question, and about the relationship between diplomacy and territorial acquisitions. Also, there is westward movement in the sense that the sections and the various Wests are considered in an east-to-west sequence, yet there is almost no usage of the word "frontier" and very little reference to the theories of the "father" of frontier history—Frederick Jackson Turner—from whom Frederick Merk inherited his famous Harvard course known to generations of students as "Wagon Wheels."

Rather, the author has delved deeply into the geographical, political, and economic development of American sections and the way in which they patterned the nation. Not unlike other authors he has included a good deal of information set forth in his earlier writings, this being the summation of his life's work, and therefore it is not surprising to find much about Texas, Oregon, slavery, and expansionism. Also there are offered some rather minute details about the economic history of Wisconsin during the 1860s. Considering the many aspects of America's westering disposition that the author did not, perhaps could not, include in his study, he might well have omitted or severely edited his extensive examination of such incidents as Wisconsin's Beef Slough War of the 1860s.

About twenty percent of the volume is devoted to the colonial period, a portion that deals in depth with the Indian background of American history and the penetration by French and English interlopers into native-held areas but very little with Spanish intrusions. This is followed by thirty chapters—out of a total of sixty-four—that are concerned with trans-Appalachian settlements and all the sectional problems earlier referred to. Except for a brief leap-frogging of the Rocky Mountains, to touch base with California and with the Utah Mormons, the reader does not view the high plains or Merk's "Cordilleran West" until he has read two-thirds of the book and, even then, the author retreats to the mid-West apparently to regroup his forces for the final assault upon the last West. Conquest and settlement of the land beyond the 100th meridian is treated tangentially and sketchily, as if the New England frontiersman regarded this as still being the Great American Desert. The book's final chapters relate to the twentieth century, with special reference to today's problems: water, strip mining, soil conservation, agricultural overproduction, migratory farm labor, Indians, and land-use planning. The latter section, like earlier sections, is topical, detailed, often technological, and generally devoid of people.

In many respects the material, concepts, and

language in this work are old and out-dated—the use of such terms as “bucks” and “squaws” when referring to Indians has long since been abandoned—but in other ways the author obviously tried to keep up-to-date and even to anticipate, since environmental problems resulting from the wastefulness of the westward movement are emphasized. The maps, old and copied (badly) from much earlier publications, are not of much use to the reader.

By comparison to the standard histories of the westward movement this is an off-trail, intriguing, encyclopedic volume, one that will be closely examined by many a young lecturer in American history or Western history for fugitive details otherwise difficult to find. It is more a source book than one of concepts. It is not for the general reader, and it has too many holes in it to be a suitable textbook for Western history courses as given today.

ROBERT G. ATHEARN
University of Colorado

JEAN-BAPTISTE DUROSSELLE. *France and the United States: From the Beginnings to the Present*. Translated by DEREK COLTMAN. (The United States in the World: Foreign Perspectives.) Chicago: University of Chicago Press. 1978. Pp. viii, 276. \$18.00.

This brief survey of Franco-American relations, “from the beginnings to the present day,” is a volume in “The United States in the World: Foreign Perspectives” series, edited by Akira Iriye. In nine crisp, summary chapters, Jean-Baptiste Duroselle, the distinguished professor of international relations at the Faculté des Lettres de Paris who is perhaps best remembered in the United States for his *From Wilson to Roosevelt: Foreign Policy of the United States, 1913–1945* (1963), interprets relations between the two countries from their colonial beginnings in America—“The Hereditary Enemy”—to a post-de Gaulle “Conclusion” attributing to both sides “indifference, incomprehension, affection.”

Since he admits to “embarking upon an almost impossible task: the reduction of the relations between two peoples, those of France and the United States, to a brief summary” (p. 1), Duroselle necessarily paints with broad strokes. The first chapter surveys the era of the American and French Revolutions, the second, the “Century of Distant Relations, 1815–1914,” and the remaining seven chapters deal with relations since 1914. Although shrewd insights are sprinkled throughout the text, Duroselle is more successful in the later chapters than in the earlier ones. For example, one wonders at the statement that Thomas Jefferson was in love

with Mme de Corny (p. 32)—did he mean Maria Cosway? Citizen Edmond Genêt could not have been, as Duroselle suggests, a friend of Jefferson’s in France (p. 36), since he was in St. Petersburg. It was certainly a slip to say that “Adams did not declare war” (p. 40), as of course he could not have, and Duroselle is mistaken concerning the terms of the Convention of 1800, which was ratified in 1801, not 1802 (p. 41). Also, Pierre L’Enfant drew up plans for the federal city in 1791, not after the War of 1812.

But these are perhaps minor defects. Duroselle is especially good on the relations between Woodrow Wilson and Clemenceau, both of whom were idealists (Clemenceau, “in the sense that he was in love with justice”) and true friends (p. 10). Inevitably, General Charles de Gaulle figures prominently. Although Duroselle emphatically refutes French critics, including de Gaulle, of the Yalta agreements—“Yalta, far from being an occasion for world partition, marked the last attempt of the Western powers to prevent it”—the General’s view of that Conference “formed the basis upon which what one might term ‘Gaullist anti-Americanism’ was firmly established” (p. 172). While President Roosevelt regarded de Gaulle as an officious prima donna, the latter never abandoned his belief that the United States aimed at a hegemony destructive of French independence. Unlike Jean Monnet, Robert Schumann, and the Americans, de Gaulle did not look toward an integrated Europe but a Europe of States. Duroselle’s summation may be just: “The irony of the Franco-American rivalry was that de Gaulle’s conceptions tended to outrun his means, while the power of the United States was superior to its conceptions” (p. 241). Despite recurrent misunderstandings, however, “Franco-American relations have never lost the mysterious charm that assures them a place apart . . .” (p. 253).

ALBERT HALL BOWMAN
University of Tennessee,
Chattanooga

THOMAS H. ETZOLD, editor. *Aspects of Sino-American Relations since 1784*. New York: New Viewpoints. 1978. Pp. xii, 173. Cloth \$10.00, paper \$5.95.

With the basic theme that “American intentions in Asia in general and toward China in particular, have been hazy, and policies and actions inconsistent” (p. vii), seven essayists touch upon a broad range of topics including not only such familiar ones as the Open Door but also the experiences of the Chinese in America and cultural contacts as a result of commercial activities. William Brinker’s essay on the latter is most interesting; he notes

that Chinese in the early years often exported lower quality goods while retaining the higher quality goods for the domestic trade, thus unintentionally misleading buyers in the West who had no idea of what really fine Chinese products were like. Frederick B. Hoyt and Eugene P. Trani describe the difficulties Chinese faced in the United States. They point out that in America, as in China, very few (685 out of 100,000 in 1885) ever became Christian. One minister wrote, "We have clubbed them, stoned them, burned their houses, and murdered them"; he wondered why there were not more conversions (p. 35). With his usual elegance of style Raymond Esthus discusses the Open Door from 1899 to 1922 and underlines the basic theme of change and confusion despite an appearance of consistency presented to Americans by succeeding administrations. David Trask deals with Woodrow Wilson and East Asia at the Versailles conference. He points out that the President was worried more about future wars in Asia than in Europe. While Wilson was extremely pro-Chinese he found that his support of the sanctity of treaties in principle ran afoul of Japanese treaties signed with other powers turning over German concessions in Shantung to Japan. The editor, Thomas Etzold, adds an excellent description, brilliantly analyzed, of the problems of developing a security policy for East Asia from 1948 to 1951 when it was difficult to assess future trends. This superb essay has a first-rate tale on the crucial positions of Douglas MacArthur and George Kennan that will dispel a few myths about the process of American policy making. The last chapter, by Jerome K. Holloway, Jr., and Etzold, deals with the contacts between Chinese leaders, such as Mao, and American diplomats like Patrick Hurley, Harry Truman, George Marshall, and Richard Nixon. On the latter, the authors stress how the Chinese used Nixon for their purposes and kept him in his place; the President, however, was also able to make great use of the China trip to bolster his political fortunes back home.

This is an excellent book, useful not only for its stimulating interpretations but also for an abundance of information that will prove helpful to both advanced and beginning students in Sino-American relations. Despite the differing topics, the volume holds together well and shows signs of good editing. The annotated bibliographies at the end of each chapter are useful. All in all it is an excellent job, and one hopes that we will be hearing more from Thomas Etzold.

THOMAS BUCKLEY
University of Tulsa

JOEL H. SILBEY, ALLAN G. BOGUE, and WILLIAM H. FLANIGAN, editors. *The History of American Electoral*

Behavior. Princeton: Princeton University Press. 1978. Pp. xv, 384. Cloth \$27.50, paper \$12.50.

This collection of essays in quantitative political history is a product of the 1973 Cornell conference on electoral behavior, sponsored by the History Advisory Committee of the Mathematical Social Sciences Board. It stands as one in a series of important volumes in quantitative history published over the last decade. The editors, Joel H. Silbey, Allan G. Bogue, and William H. Flanigan, have structured the essays around four major themes: partisan eras, popular participation, determinants of electoral alignments, and the policy impact of the vote. They offer an introduction to each section and in the initial essay provide a cogent overview of the development of historical electoral behavior as a research field. Even five years after the conference, the essays are a welcome addition to the literature. And four or five of them are important to further work in the field.

The partisan phasing of electoral alignments opens the volume. In that section's first essay Walter Dean Burnham, Jerome M. Clubb, and William H. Flanigan develop a national realignment-equilibrium model using state-level voting returns from 1828 to 1964. They extend their discussion and the data to party control of the presidency, the Senate, and the House. Thus, the essay is cast broadly over the national terrain, and realignments emerge as pervasive, system-wide crises, productive of shifts in partisan control in the electorate and in the policy arms of government, leading to "innovative policy actions" (p. 70). The second essay, by Lee Benson, Joel H. Silbey, and Phyllis F. Field, considers some of the same issues within New York state from 1795 to 1970, using county-level voting returns. These authors argue that state realignment eras did not recapitulate national ones, that partisan cycles were not neatly generational, and that electoral order actually revealed "a high degree of disorder" (p. 103). Neither essay speculates on a model of partisan micro-dynamics linked to the macro patterns each describes. The specification of such a linkage remains a lacuna in the literature.

The final essay of the first section is Nancy Zingale's clear and cogent case-study of Minnesota's third party alignments between 1888 and 1950, using county-level returns. Zingale considers the absorption of the Populist party by the Democrats in the 1890s and the replacement of the Democrats by Minnesota's Farmer-Labor party after World War I. She argues that a third party can survive for a sustained period, as did Minnesota's Farmer-Labor coalition, when the local political agenda differs from the national one.

The three essays in section two are linked by a

focus on political participation. David Bohmer offers new and exciting data—poll books surviving from two Maryland counties between 1796 and 1802—by which he can trace individual voting participation and partisan choices in the first party system. This individual-level data directly disproves the received opinion that partisan loyalties were weakly developed in this early period. Bohmer finds that aggregate fluctuations in the partisan vote did not rest on shifting partisan preferences among voters in these counties but rather on varying participation among voters with highly stable preferences. This finding is critical to the reassessment of earlier party systems and to the task of linking micro and macro models of voting behavior. The second essay in this section is equally important. Jerrold G. Rusk and John J. Stucker show how the impact of literacy tests and poll taxes sent declining Southern participation rates plummeting precipitously from 1890 to 1916. It complements J. Morgan Kousser's book and illustrates important conceptual and measurement issues of quasi-experimental research design. The last essay of this section, by William Nesbet Chambers and Philip C. Davis, describes the shift in state-level turnout from low to high between 1824 and the 1840s in association with increased party competition and the emergence of centralized national party organizations within the states.

Section three considers determinants of the vote, and section four the effects of the vote on urban public policy. Martin Shefter describes how New York City's rampant factionalism prior to the late 1880s was transformed into Tammany's machine control of ethnic voters and Democratic politicians. He uses an ecological regression model to mark the emergence of strong ethnic support after 1888 but argues that social compositional factors alone are inadequate to explaining the power of the urban machine. The next essay is a re-examination by Robert R. Dykstra and David R. Reynolds of Michael P. Rogin's argument for a left-of-center agrarian progressive tradition in Wisconsin. They reject Rogin's views and further underscore "the fallacy of too rigorous an application of the 'pietistic'/'liturgical' dichotomy" (p. 321). Yet their study has its own methodological difficulties in the face of which their factor analysis proves unmanageable. The final essay in this section is an important empirical treatment of ecological inference by the late John L. Shover and John J. Kushma. It throws no new light on the mathematical theory of such inference but suggests facets of its application well worth considering, such as the level of aggregation and the use of weighted versus unweighted estimates.

The final section contains the single essay by J. Rogers Hollingsworth, who sought to measure

the impact of electoral behavior on public policy across 200 small cities. He concluded that regularly scheduled elections showed no "consistent and systematic influence on policy outcomes" (p. 370). As a whole, the volume is a valuable marker to the work in this field and will be of continued interest to specialists.

MELVYN HAMMARBERG
University of Pennsylvania

MAX J. SKIDMORE. *American Political Thought*. New York: St. Martin's Press. 1978. Pp. xii, 260. Cloth \$12.95, paper \$5.95.

This volume attempts briefly to describe the major forces in American political thought as well as what the author calls crosscurrents running counter to the main themes. Written by a political scientist at Southwest Missouri State University, the book is intended as core reading in courses surveying political ideas in the United States. While instructors in political science must judge Max J. Skidmore's success, students and teachers in American history will find this book has limited merit. Using mostly secondary accounts, many of which should have been supplemented by more recent work, the author traces the nation's political ideas through broad topics placed in chronological sequence. After a beginning of barely thirty pages devoted to such influences as Classical, Judeo-Christian, Reformation, and English thought, and to the impact of America's colonial experience, the book arrives at the American Revolution. There follow sections on the federal Constitution, the early Republic, the Jackson era, the "Romantic Response," rising national power, slavery disputation, the industrial and progressive eras, the interlude after World War I, the depression and its aftermath, and, finally, the contemporary scene. Footnotes and a listing of recommended additional reading are placed at the close of each of the fourteen chapters.

Skidmore uses summaries of American history as background for allusions to numerous American thinkers, most of whom necessarily are noted almost in passing. He takes great pains to reconstruct the early presence of today's concern over Indians, blacks, and women, and there is a hortatory element in his hope that he may have contributed to strengthening American political philosophy so that public action might more ardently pursue goals most historians would describe as "Progressive." In both design and spirit, the book shelters the haunting presence of Vernon Louis Parrington, whose noble work Skidmore mentions often in his own discovery of an American past in which advancement comes through the thoughts

and deeds of those who champion popular democracy, guided capitalism, and the redress of minority oppression.

While all this may or may not make the book helpful to political scientists, for historians there are serious problems. Aside from the volume's simplistic—even naive—portrait of American history, the greatest difficulty stems from the author's allowing "liberal" political activism to be the fundamental concern so that even the most familiar contributors to American thought are transitory. Among the curious results, Frances Wright receives larger attention than do *The Federalist* essays and A. Philip Randolph emerges as more important for American political thought than does the entire Adams family. These and many other imbalances are no doubt attributable to Skidmore's determined mixing of "crosscurrents" with the major ideas. Another weakness accompanies what may be the author's limited acquaintance with historiography—both in matters of evidence and scholarship. Arthur Schlesinger, Jr., is described as "one of the few contemporary writers who has recognized the moral dimensions of the slavery controversy" (p. 138), an evaluation based on Schlesinger's 1949 article in *Partisan Review*. Walt Whitman's brooding *Democratic Vistas* is ignored while the poet is mentioned as the solitary Transcendentalist to preach "an abiding democratic religion" (p. 112).

Faced by enormous difficulty in writing a book of this small size and wide scope, Skidmore's achievement rests in an occasionally apt summary of someone's thought, which happens in the case of Reinhold Niebuhr, for instance. Most readers will not find this enough for satisfaction.

PAUL C. NAGEL
University of Georgia

G. EDWARD WHITE. *The American Judicial Tradition: Profiles of Leading American Judges*. New York: Oxford University Press. 1976. Pp. x, 441. Cloth \$15.95, paper \$4.95.

Sketching the personalities, careers, and achievements of twenty-eight American judges from John Marshall to John Marshall Harlan the second, G. Edward White delineates the contours of a "tradition" of American appellate judging. He characterizes the tradition as containing three elements: "a tension between independence and accountability, a delicate and unique relation to politics, and a recurrent trade-off between acknowledged powers and freedoms in the individual judge, and acknowledged constraints on the institution of the judiciary" (p. 2). Although White's individual portraits are often perceptive

and vivid, his concept of the "tradition" is unsatisfying. The "elements" seem not only overlapping and undifferentiated among themselves but also, taken together, either amorphous or obvious. Without intending a mere semantic dispute, one might acknowledge that those elements sketch part of a condition but deny that they constitute a tradition. On the one hand, White's tradition is so general that it homogenizes the diversity among American judges. His analysis requires sharper categorizations that take us beyond the vague concept of a tradition randomly marked by "dialectics" and "mosaics." On the other hand, insofar as there is (or may be) an identifiable and vital tradition of American judging, it would most likely be found in deeply ingrained and often unarticulated social, cultural, and professional assumptions that operate at the level of a guiding perceptual framework. Such assumptions, and the conditions that produce and reinforce them, receive little attention. White's judicial tradition, in other words, does not seem to adequately explain many striking differences or to sufficiently illuminate basic underlying similarities.

The book, nevertheless, has many virtues, not the least of which is its refreshing readability. It will be informative and enjoyable for the professional as well as for the interested amateur. The portraits are lucid, salient, and well focused, and they readily suggest the variety of ways in which judges have exercised the personal discretion permitted by the institutions of the law. Additionally, the book's broad sweep is admirable. Avoiding the classic danger of legal history, missing the tree for its bark, White highlights significant legal developments while examining the changing self-conceptions of American appellate judges.

The method of collective biography seems useful for exploring relationships among personalities, professional techniques, and social forces. One central problem of the newer legal history is to develop methods of integrating broader social forces with the sinews of the technical judicial process and of showing in specific terms how, where, and why they interact and what particular effects each has on the other. Such a method can help avoid the shallow and generally bland conclusions of much mechanical behavioral research.

Although that approach suggests fruitful possibilities, *The American Judicial Tradition* does not always fulfill them. The book recognizes the general significance of social factors and discusses in some detail the influence of intellectual changes on the inner traditions and processes of the law, but it fails to consider adequately the economic, social, and cultural developments that inform and give concrete meaning to those traditions and processes. It retains the excessive emphasis on both the

language of appellate opinions and the centrality of the "reasoning" judge which has, by almost universal recognition and practice, distorted our understanding of the law. The major significance of legal history lies in its analysis not of doctrine and technique but rather of functional relationships between the legal system and the society that supports it. Ultimately any interpretation of the law must draw its meaning from a broader theory of society, and *The American Judicial Tradition* would seem still to be rooted in the dominant official view of the contemporary American law school.

Inevitably such a panoramic book will provoke a variety of disagreements. It seems likely, for example, that White has overemphasized and oversimplified the "oracular" judging style of the nineteenth century. Additionally, focusing on "great" judges and decisions may well distort the nature of a tradition which for the most part consists of ordinary judges making ordinary decisions. One wonders what points of divergence would arise if one compared White's interpretations with conclusions drawn from a broad sampling of what Karl Llewellyn called a "mine run" of cases. Again, of the judges studied, two-thirds were members of the United States Supreme Court. Not only does this tend to continue our overemphasis on both abstract doctrine and its pristine formulator, but it also obscures the extent to which the tradition of "the Court" is different, necessarily or otherwise, from that of other appellate tribunals.

Regardless of problems, however, *The American Judicial Tradition* is valuable and enlightening. The individual essays are frequently excellent, and the herculean effort of producing a general overview should enrich our scholarship. White has written a thoughtful and often provocative work that both synthesizes many recent contributions and suggests further lines of inquiry. Like Lawrence Friedman's *History of American Law*, it will provide a challenging starting point for much future historical writing.

EDWARD A. PURCELL, JR.
Harvard University

RICHARD S. PATTERSON and RICHARDSON DOUGALL.
The Eagle and the Shield: A History of the Great Seal of the United States. Washington: Department of State. 1976. Pp. xlv, 637. \$12.00.

Richard Sharpe Patterson and Richardson Dougall were colleagues for two decades in the Historical Office of the Department of State, and the second author saw the "basic manuscript" through to publication after Patterson's death in October of the Bicentennial year. Dougall is a true

coauthor, having logged many hours and miles in supplemental research and revisions until June 1977. As a history, the book is a very readable encyclopedia of everything anyone would wish to know, from factual or esthetic points of view, about the Great Seal. No one reading this review needs a library to know what this palladium of the United States looks like. Since the last half of 1935, the Great Seal (reverse left, obverse right) has appeared and has been so labeled on the back of our dollar bill. The story of how Henry A. Wallace saw a color plate of the reverse in Cordell Hull's outer office, went to President Roosevelt to suggest a coin, and saw his leader work out all the details until our smallest denomination of paper assumed its present form is one of the fascinating vignettes excavated from a dozen government files and collections of private papers.

Since the Great Seal is in the custody of the Secretary of State and is used nowadays primarily for international documents or commissions relating to our representation on most levels abroad, this publication takes a very natural place among those of the Department of State, in this instance also bearing the seal of the American Revolution Bicentennial. Otherwise, this book is on a par with the best of what one might expect from a historical society or a well-subsidized university press. The Great Seal, like the Declaration, was born on July 4, 1776, although it took until June 20, 1782 for Congress to adopt the device in its present form, which has been modified visually according to generations of refining taste down to the Max Zeidler die of 1904. The press and cabinet, including new counter-dies, have been updated in the past seventy-five years, but the die itself has not been renewed. The new Department of State received the Great Seal die and press as the result of an act approved September 15, 1789 and has been custodian of our highest official instrument ever since.

As the title of this book suggests, it is the obverse of the seal which is best known and has been most used, from the Diplomatic Medal of 1792 to the insignia on army officers' caps to a host of private motifs in every conceivable medium and from gilded wood (ships' fittings) to glass or china and to works of art on paper (William Reimann's recent designs for the decoration of a major Boston bank building). In the wealth of facts, sources, biographies, analytical bibliographies, and illustrations provided here, the authors make very clear what we were spared when suggestions of the Founding Fathers were shelved in the early committees for the Great Seal. John Adams proposed the Choice or Judgment of Hercules, a holdover from the Wildman phase in northwest European heraldry, and Thomas Jefferson could have given us the Children of Israel in the Wilderness on the obverse

and the Anglo-Saxon pioneers Hengist and Horsa, or Pharaoh being overwhelmed while Moses and the Israelites watch from the shore, on the reverse.

CORNELIUS VERMEULE
*Museum of Fine Arts, Boston, and
Boston College*

WALTER BLAIR and HAMLIN HILL. *America's Humor: From Poor Richard to Doonesbury*. New York: Oxford University Press. 1978. Pp. xvi, 559. \$17.50.

Not minding that writers on so subjective a topic as humor are an "endangered species" (p. vii), Walter Blair and Hamlin Hill, two long-established scholars in this field, have offered an ambitious overview of American humor in which they have treated "even humor that we think unfunny, provided it was influential, typical or widely enjoyed" (p. vii). Besides canvassing fiction, verse, and the stage, these intrepid explorers have ranged through jokebooks, political pamphlets, almanacs, folklore collections, and many periodicals; they have also drawn upon radio, cinema, and television. Their approach is primarily historical and cultural, but at intervals they offer intensive analyses of especially evocative and representative works—for example, Benjamin Franklin's war satires, "Rip Van Winkle," T. B. Thorpe's "The Big Bear of Arkansas," James Thurber's "The Secret Life of Walter Mitty," W. C. Field's film *The Bank Dick*, and the stand-up comedy of Lenny Bruce—from a basis mainly formal and esthetic. (I hope the authors will forgive the use here of those highfalutin terms; their own style is lively and informal though not less scholarly for being eminently readable.) Through this two-part approach, Blair and Hill have tried to take a fresh look at their subject; and their book, though short on basic theory, is long on contributions to specific aspects within its scope.

One contribution is the broadening of Franklin J. Meine's term "folk literature" into the concept of "folk journalism," a rubric that enables Blair and Hill to make constructive distinctions between oral and printed humor as well as to deal with humorous material about historical figures like Davy Crockett and mass media creations as different as Paul Bunyan and Doonesbury without getting sidetracked by controversy about historical validity or about the relative status of folklore and "fakelore." Another contribution is the interpretation of many humorous confrontations in the work of Mark Twain and several other writers as versions of the ancient conflict between the *alazon*—the braggart, the person who seems to be more than he is—and the *eirone*—the modest imposter, the person who seems less than he is. Other

scores include emphasis on the Old World sources of much supposedly indigenous humor; investigation of how oral storytelling has continued to influence fiction right down to the present; detailed inquiry into the crosscurrents of mutual influence pervading film, television, radio, and the print media; and a pregnant "Afterword" that includes provocative discussion of the humor of Will Rogers, Harry Golden, and Harry S. Truman.

Stress on the evolution of humorous genres and on the achievements of specific humorists may somewhat limit the use of this book for some historians. For instance, they will not find chapters directly on the humor of historical events like wars or the depression of the 1930s or on conventionally designated periods like the Jacksonian era or the 1920s. However, they will find a many-faceted introduction to the history of a phenomenon pervasive in American culture.

NORRIS YATES
Iowa State University

JOHN A. LUCAS and RONALD A. SMITH. *Saga of American Sport*. Philadelphia: Lea and Febiger. 1978. Pp. x, 439. \$16.50.

This is a useful study of sport in America from the colonial period to the present. Its major source of strength is that it is concerned not merely with the history of sport, but with sport as a reflection of the nation's dominant social trends. The book lays heavy stress upon the continuity and divergency of American society which nourished and expanded America's love of sport.

A source of weakness in the book is that insufficient attention is given to placing ideas about sports in a meaningful context. The method employed moves the saga from period to place to type of sport without analysis or evaluation. Only a specialist in the field is likely to draw meaningful conclusions from this factual narrative. The authors start off with some challenging hypotheses but fail to apply them with any consistency or coherence.

The book is divided into three parts: (1) colonial and early American sport; (2) American sport in transition, 1865-1900; and (3) sport in twentieth-century America. In the course of the narrative, we learn that Puritanism greatly influenced "the introduction of voluntary school competitive athletics and compulsory physical education" (p. 37); that throughout the nineteenth century most Americans regarded sport as a waste of time; that "winning" was an early sport attitude; that the rise of intercollegiate athletics was the most unique American sports feature of the nineteenth century; and that World War II broke down the prejudices

of the past centuries against women and blacks in athletic competition. There are separate chapters on women and blacks in sports, which, rather than encouraging an American audience to deprecate past practices, tends instead to reinforce the segregationist and discriminatory practices so prevalent in an earlier period. While this may be offensive, it is not startling or provocative.

It is a good solid work, however, well researched and concise in its factual presentations. *Saga of American Sport*, however, lacks insight and analysis. Nowhere in this work do John A. Lucas and Ronald A. Smith draw any meaningful conclusions from their findings. Indeed, the major flaw of this book is the absence of a concluding chapter drawing together all the disparate elements that went before. Each chapter is written as if it were an article and not part of a larger work; the only common denominator is that each chapter details a distinct sporting development without thought of its connection with the preceding chapters.

The work adds little to our knowledge of sports in the United States and, sadly, fails to build on the great tradition established by John R. Betts in *America's Sporting Heritage* (1974). Nevertheless, the book has some good points: it is highly readable, well written, concise, and factual. It provides a great deal of useful information on sports, is a worthwhile introduction to the field, and should provide enough stimulus for enterprising students to go further in their evaluation of America's sporting heritage and its place in the larger society. It should be read by all serious students of American cultural and social history and is worthy of inclusion in any general studies course on American life and culture.

ROBERT P. SMITH
Department of the Army

DAVID SCOFIELD WILSON. *In the Presence of Nature*. With an introduction by GENE WISE. Amherst: University of Massachusetts Press. 1978. Pp. xix, 234. \$15.00.

This imaginative, interpretive essay by an American Studies scholar concerns the natural history work of Jonathan Carver, John Bartram, and Mark Catesby. It draws on an old theme: the natural history of the New World jarred the assumptions and eventually the cosmos of Europeans and Americans. David Scofield Wilson seizes on the long-recognized point that some seventeenth and eighteenth century naturalists were no more than collectors while others systematized and rationalized the new knowledge within a new cosmos. The former Wilson calls nature reporters; their work was nature reportage. The latter were

natural philosophers and historians. Carver, Bartram, and Catesby were nature reporters, and their potency lies "in their demonstration of how to survive and thrive within, even deflect somewhat, whichever culture project is in fashion" (p. 44). Wilson has particularly high praise for Carver, who became a secular prophet. A major question raised in the Carver chapter is: "What prepares him [Carver], allows him to turn himself into a politician of the cosmos and weld into a plausible coalition the diverse rhetorics of economic, spiritual, national and natural interests? This is a question of two dimensions, one of evidence, one of argument" (p. 60).

The book also contains twenty-four illustrations from Catesby's *Carolina* (reproduced, unfortunately, in black and white), a poem by Wilson, an epilogue discussing nature reporters' balm for twentieth-century malaise and the potential of American Studies as a discipline, a personal narrative describing how the book came to be written, endnotes, and a serviceable index. The Introduction by Gene Wise of the University of Maryland speaks of the book in glowing terms—a powerful, wise, disciplined, mature one which "stands firmly in the humanities, then, but its tools of analysis build a bridge to the social sciences" (p. xix).

I do not agree with those judgments. The theme has been worked for years. The distinction of nature reporter and natural historian does not seem to have much potential for development—it was not important to the eighteenth century; it is not an exclusive classification, for (perhaps unknown to Wilson) Bartram both collected and systematized. There is also some difficulty with the subjects. Wise observes they were "three rather undistinguished figures of the eighteenth century" (p. xiv). Despite Wilson's work, if I did not know otherwise, I would continue to hold them all in that esteem. Carver's greatness rests more on argument than evidence; no one argues Catesby was more than mediocre. But if Wilson had not relied so heavily on printed sources and had turned to the manuscripts of Bartram and his correspondents, a deeper understanding of Bartram might have been reached. Despite passages of rhetoric and attempts at social, psychic, and sacral analysis, how the book bridges the humanities and social sciences is not clear; and it seems thin in structure, sources, and analysis. It is a labor of love created over several years work. But it may fall short of expectations raised in the introduction; those who approach it for new insights into eighteenth-century European-American science may be disappointed; and its interpretations may not convince all.

GEORGE E. BATES, JR.
Winona State University

HENRY STEELE COMMAGER. *The Empire of Reason: How Europe Imagined and America Realized the Enlightenment*. Garden City, N.Y.: Anchor Press/Doubleday, 1977. Pp. xiv, 381. Cloth \$10.00, paper \$3.95.

Until the appearance of this book and of Henry May's *The Enlightenment in America* in 1976, our ideas of the subject were strangely nebulous and our information scattered among such different branches of knowledge as the histories of science, religion, and politics. Henry Steele Commager's new book is thus primarily an essay in definition and unification but also in contrast: the chronological and analytical categories of the American Enlightenment that occupy May are of less concern to him than its overall character and the resulting contrast with Europe.

When enlightened ideas spread to America they carried the same spirit of liberated inquiry, impelled by the conviction (the faith, dare we say?) that all questions concerning man and the material world are answerable. But in America they encountered entirely different conditions, which helps to account for the different consequences to which Commager attaches such overwhelming importance. Those colonials whose reading extended to the most advanced thinkers in Scotland, France, and England faced certain obvious disadvantages: they had no great libraries, no dissenting academies, few intellectual centers (though Commager emphasizes the quality of intellectual life in Philadelphia), and no laboratories. But there was a countervailing advantage: no great enemies. Although the colonies contained inherited institutions of Old World character, these almost nowhere exercised comparable powers, and the enlightened class also formed a higher proportion of the class from whose ranks legislators were drawn.

Commager's account is packed with information but never congested; he communicates a sense of constant and significant development and of almost youthful excitement. But his intense interest in science and literature does not hold back the central themes, which culminate in the convergence of American enlightened thinking and action in politics. "The Americans were more politically minded than any other people. The whole of their philosophy—now that Jonathan Edwards was out of the way—was really political philosophy, and the whole of their literature, too, or all of it that counted, in the long run" (p. 112). For Edwards's disciples, thus brusquely dispatched, we must turn to Henry May. But Commager and May agree in converging on the election of Jefferson as an epitome of the American Enlightenment.

Two powerful—and not in my opinion incompatible—views of political development have affected recent thought. One deals with the trans-

mission of ideologies, the other in institutional history—the handing down of practical, acquired custom. Where, then, does Commager stand? There seems here at first sight to be something of a lacuna in his explanatory methods. Clearly his interests are not primarily institutional. The answer must be sought in another convergence, of enlightened ideas with favorable circumstances. And here Commager can best be understood when we grasp the fact that he is a child of exactly that humane optimism which assumes that people will behave in enlightened ways when benevolent theory strikes friendly ground—which, for a multitude of familiar reasons, Britain's American colonies provided (but not those of France, Spain, or Portugal: the contrast seems as valid as that between Europe and America, on which he insists).

On occasions his protonational enthusiasm gets the better of him. He really seems (p. 204) to believe that the colonials wanted to free themselves "from the curse of slavery" rather from advancing racial imbalance through the Atlantic slave trade and that colonial governments were founded on a series of free compacts (p. 178). Nor is he impressed by recent evidence of stratification. An important section of the argument turns on the American advance in federal government. In Europe, enlightened political thought concentrated on central power, in America, on distribution. It was a difference of basic circumstances that became a profoundly different principle in the manifestation of self-government. The great American achievement was the advance in the use of representative institutions to hold a federal republic together.

The keynote of this infectiously readable book is in its preface: "We take for granted what neither Americans nor Europeans took for granted in the eighteenth century; not only have we lost that sense of astonishment and exultation that animated Jefferson's generation, we have almost lost our ability to understand it" (p. xiii). Commager restores these lost gifts.

J. R. POLE
*Woodrow Wilson International
Center for Scholars,
Washington, D. C.*

WILLIAM C. LEHMANN. *Scottish and Scotch-Irish Contributions to Early American Life and Culture*. (Series in American Studies.) Port Washington, N.Y.: Kennikat Press, 1978. Pp. viii, 232. \$9.95.

In the seventeenth and eighteenth centuries, many thousands of people migrated from the northern parts of the British Isles to the American colonies. The economic and political reasons for that movement have been well described. The actual settle-

ment of these people of Scottish origin in the New World is a well-known part of our American history. This new study by William C. Lehmann analyzes the characteristics of the Scots and Scotch-Irish and the way in which these settlers influenced the society of which they became a part.

Lehmann believes that there was something distinctive about these immigrants and that as individuals and as groups they had a seminal effect upon American culture. The fact that they prized intelligence, education, order, reason, and scientific inquiry made a big difference in the way in which these people influenced their new American neighbors. Lehmann describes with considerable detail the impact of the Scots and the Scotch-Irish on the American economy, medical practice, religion, education, science, philosophy, letters, law, and politics. He stresses the Scottish character of academies and colleges in colonial America. He documents the relationship between the Scottish universities, American medical practice, and the establishment of scientific societies in the New World. He points to the remarkable impact of the "Scottish Enlightenment" on New World culture. The American Revolution is tied in many ways to the ideas and activities of these Scottish and Scotch-Irish immigrants.

The book makes a special contribution to the literature by "wrapping in a single blanket" the histories of several groups usually considered separately—the Ulster-Scots, the Lowland Scots, the Highland Scots, and the Island Scots.

Lehmann believes that those qualities which were of the greatest importance in the building of the new culture were characteristics that the Scots readily provided. For example, he stresses the importance of reason and intelligence in Scottish piety and religion. The emphasis on reason gave the Scots an advantage. Consequently, they had a major impact on religion and religious thinking in many churches beyond the confines of Presbyterianism. The parts of the book that are particularly illuminating are those related to the practice of medical science in the colonies and the conduct of education. The book is also distinguished by its description of Scottish Calvinism and its impact upon American religious development. Those chapters dealing with philosophy, letters, and law are not as detailed.

Lehmann says that he is not "a professional historian." He is, however, a sociologist who writes good history. To reinforce his claim to objectivity, he notes that he does not have Scottish origins and he is not Presbyterian. He is sensitive and cautious in his generalizations regarding groups. Although he holds a high regard for the Scots, his book is written with restraint. His recognition of the important role that the Scots played

in the formation of American educational institutions and cultural standards does not keep him from recognizing the other influences that came to bear in American development. Lehmann has written a fine monograph. It is a good addition to our literature describing early American history.

DUANE MEYER

Southwest Missouri State University

JEFFREY J. CROW and LARRY E. TISE, editors. *The Southern Experience in the American Revolution*. Chapel Hill: University of North Carolina Press. 1978. Pp. xvii, 310. \$15.00.

By the basic canons of historical scholarship—thorough research, meaningful interpretation, and lucid expression—two of the essays in this collection, Michael Mullin's "British American and North American Slaves in an Era of War and Revolution 1775-1807" and Mary Beth Norton's "What an Alarming Crisis is This": Southern Women and the American Revolution," are superb. Both authors explore the impact of revolutionary violence on their respective subjects. Mullin's broad focus encompasses all of the southern Atlantic's Anglo-American slave society. Basically, he asks why peace generally prevailed among the vast majority of the slaves during the revolutionary era and why forms of resistance that did occur never escalated into a revolution on the scale of St. Domingue. Mullin demonstrates how the major staples—sugar, rice, and tobacco—each precipitated a distinctive social system. On the Sugar Islands, white absenteeism, the presence of black managers, and the adoption of the task system of production allowed slaves to dominate the islands' internal economies through the growing and marketing of foodstuffs. Upon that basis the slaves carved out a substantial zone of independent existence that ensured the transmission of African culture and fostered the acquisition of property. Mullin suggests a similar pattern for the rice regions of South Carolina, as does Peter Wood, whose remarks on the black experience in revolutionary South Carolina, though partly subsumed by Mullin's essay, provide a sensitive exposition of one brief moment. The tobacco-growing Chesapeake, by contrast, witnessed the rise of a patriarchal planter economy where white masters controlled most segments of daily life. As a consequence, the sugar and rice plantations constituted areas of autonomous survival and refuge during the era of violence, while in the Chesapeake the war's disorder undermined patriarchal authority and encouraged the region's less rooted slaves to flee, most often to no avail, as disorganized fugitives.

Mary Beth Norton studies the effects of revolutionary violence on Southern women. After a concise discussion of the circumscribed and relatively secluded life of most women as dictated by their familial roles, the author vividly conveys their changed condition during the war, carefully noting the differential impact by race and class. For slave women, the revolutionary turmoil offered a slim hope for freedom, but for most white women it meant living with terror and coping with survival. Their struggles and the war's desolation, Norton suggests, combined to bequeath an ironic legacy. Having surmounted adversity, Southern women began subtly to challenge the confining aspects of their traditional roles, but unlike their more successful Northern counterparts, they were unable to capitalize on their wartime experiences. The sheer level of devastation in the South precluded that region's "experimentation with new ideas and new forms" and instead required the active participation of all in the restoration of the "patriarchal society."

The South's pervasive wartime violence is well accounted for in essays by John Shy and Clyde R. Ferguson. Shy perceptively depicts the British strategy after 1778 to exploit the South's loyalist sentiment and the dilemmas that Clinton and his subordinates encountered. Pursuing a policy aimed at restoring the crown's authority through loyalist occupation, the British invasion created a context of violent confusion so enormous that by the spring of 1781, none of the commanders "any longer knew what he was doing." Ferguson's well-researched study interprets the roles of the patriot and loyalist militias within this disorder, arguing that the Whigs were more effective because of the experience they had gained prior to 1779. Whether they were more representative or popular than their adversaries, as Ferguson seems to imply, remains undetermined. As he remarks, however, had the British given massive support earlier, the outcome "might have been different."

In explaining the South's disorder, both Shy and Ferguson concentrate exclusively on the conditions produced by the war. Although this emphasis is proper within the confines of their essays, a comprehensive understanding of the revolutionary South requires linking its prewar and wartime experiences. In this regard, only one essay, that by Marvin L. Michael Kay and Lorin Lee Cary on the North Carolina Regulator uprising, is helpful. The authors argue convincingly that intrasectional class tensions were a fundamental source of that rebellion. However, whether the Regulators, who had ample reason to resent the more privileged, viewed the confrontation in the explicit class terms that Kay and Cary ascribe to them is neither proven nor important.

North Carolina's sharp divisions contrast vividly with Virginia, a colony that Jack P. Greene's creative imagination describes as a "political nirvana" because of its alleged harmony between 1720 and 1760. Greene suggests that Virginia's coherence was founded upon several commonly held major values and that the most important of these—personal independence and virtue or selflessness—were simultaneously challenged in the 1760s by Parliament's new taxation program and the Robinson and Chiswell scandals that symbolized an insidious process of internal moral decay. In this context, Greene argues, an impulse of moral regeneration emerged to purify the colony—a reformist impulse activated, he insists, by the elite's devotion to Virginia's traditional ideals and not by its desire to retain power.

Where Greene examines societal characteristics, Robert Weir explores the relationship between family life in the plantation colonies and the unique nature of personality development in that area prior to the Revolution. His argument conflicts directly with Pauline Maier's essay which, in a thesis that may prove difficult to sustain, argues for a lack of regional distinctiveness on the part of the South's revolutionary leadership. Weir constructs a modal personality that is based on the assumption that a uniformity of experience produces a uniformity of outcome and includes a series of childhood and adolescent antecedents which, he contends, contributed to the development of a rebellious personality. Although the elements that Weir lists could just as easily explain the emergence of a dependent and insecure personality, he defends his investigation into human emotion by arguing that self-interest is an insufficient explanation for understanding the behavior of South Carolina and Georgia Whigs who were exceptionally vulnerable to a host of external and internal adversaries. Their emotions must be explored, Weir asserts, since by supporting independence they performed an act "virtually tantamount to suicide." No one can argue with that.

RONALD HOFFMAN
University of Maryland

MORTON WHITE. *The Philosophy of the American Revolution*. New York: Oxford University Press. 1978. Pp. xii, 299. \$15.95.

Morton White begins this book with the statement that the Founding Fathers "did not invent a single idea that may be called philosophical in the philosopher's sense of the word" (p. 3). *The Philosophy of the American Revolution* is therefore a canvass of European philosophical thinking which he considers relevant to understanding "the epistemology, me-

taphysics, philosophical theology and ethics upon which the revolutionaries rested their claim to independence" (p. 3). The philosophical themes are developed at some length. Three chapters on the epistemology of "self-evident truths" trace the antecedents of Locke's rational intuitionism back to Aquinas, discuss Hutcheson's theory of moral sense, and expound upon Burlamaqui's synthesis of Locke's and Hutcheson's ideas about the sources of ethical knowledge. Three more chapters explore the "metaphysics," "philosophy," "theology," and "ethics" of natural law and natural right. Here White goes back to Aristotle, though he puts the emphasis on later thinkers, beginning with Richard Hooker and again culminating in Burlamaqui. Burlamaqui, indeed, emerges as the synthesizer of European philosophical ideas on whom American thinkers drew most heavily. In part, these chapters deal with the reception of philosophical ideas by the revolutionary leadership, but the subject is clearly secondary. The Americans are seen only as the vehicle for European ideas, and White explicitly limits his discussion to a few major figures. Among these, Jefferson, particularly in his role as author of the Declaration, commands by far the most attention, though brief fragments from the writings of John Adams, Hamilton, and James Wilson, are also discussed, while Madison and Franklin receive a passing mention.

Readers familiar with Bernard Bailyn's *The Ideological Origins of the American Revolution* (1967) will find that White has shed little light on the question of how the "philosophy of the American Revolution" relates to its ideology. Early on, White takes exception to Bailyn's claim "that the American Revolution was above all else an ideological . . . struggle," protesting that he is "unable to understand" the "difficult words" "'above all else'" (p. 6n). Later, in a chapter entitled "The Nature of Rights," he pronounces it impossible for the revolutionaries "to have held—as Professor Bernard Bailyn says they held—that 'the sphere of power' and 'the sphere of right' were 'innately opposed'" (p. 192). In fact, Bailyn wrote "the sphere of liberty or right," a phrase which White does quote correctly on a subsequent page. But he also there reiterates his disagreement with what students of Bailyn's work will recognize as the crux of his analysis of opposition ideology. White insists that the "revolutionaries thought that physical power was neutral and that it was not evil as such" (p. 191-92). Bailyn would find no difficulty in accommodating White's point, but White apparently finds it impossible to accommodate Bailyn's. White's unwillingness to conceive of his essay as complementing Bailyn's work rather than challenging it means that students of revolutionary

thought may be led to conclude that they cannot accept one without rejecting the other.

Those interested in the historical reconstruction of American revolutionary thought will, I suspect, continue to find Bailyn persuasive because he works with the entire corpus of revolutionary literature. By contrast, White focuses on a few sentences in a few documents and loads them with elaborately defined meanings derived primarily from sources outside the immediate revolutionary context. For example, the most explicit evidence we have of Jefferson's philosophical intentions in the Declaration comes from a letter written to Henry Lee almost forty-nine years after the fact. Most of White's argument depends on inferring congruent meanings from phrases in the Declaration that echo phrases in the writings of several European thinkers whom Jefferson and the other Founding Fathers seem to have studied. Even accepting this method as conclusive, it would only show the intentions of one or a few men in relation to parts of one document. White thinks it appropriate to concentrate so closely on the Declaration because of the prestige that this document subsequently acquired. His intentions in this essay go beyond historical reconstruction to an exposé of the political uses to which ambiguous notions in political philosophy have been put. He repeatedly identifies the "jokers" in such concepts that permit ideas developed in the service of some worthy cause to be turned against it, and he concludes with a warning against uncritical acceptance of political or moral claims masquerading under the guise of self-evidence. Those who share his political and moral preoccupation with the misuse that has been made of certain notions in political philosophy will find his book more cogent than those who turn to it for an historical understanding of revolutionary thought.

RICHARD BUEL, JR.
Wesleyan University

J. B. HARLEY *et al.* *Mapping the American Revolutionary War*. Chicago: University of Chicago Press, for the Hermon Dunlap Smith Center for the History of Cartography, The Newberry Library. 1978. Pp. viii, 187. \$25.00.

The five essays in this book, which were first presented in a symposium at the Newberry Library in 1974, consider both the contemporary and the retrospective mapping of the Revolutionary War. In three of these essays J. B. Harley urges a new approach to studying maps contemporary to the Revolution. Rather than follow other historians of cartography in emphasizing where maps were made, who made them, or how they were distributed, Harley prefers to consider how maps were

used and how they affected decisions. He begins by proposing that maps used in the Revolutionary War be classified according to their use (plans of fortification and maps of reconnaissance, to cite two examples). He then demonstrates that American, British, and French commanders shared a common tradition of military cartography and that all believed maps and mapmaking essential to waging war. But Harley does little to tie maps to particular decisions. He suggests merely that maps may have misled both sides; for example, they may have lured the Americans to Quebec in 1775 or the British to Saratoga in 1777.

By contrast with Harley, who emphasizes the use of maps during the Revolution, Lawrence W. Towner and Barbara Bartz Petchenik are concerned primarily with analyzing the quality of maps in histories of the Revolution. Towner concludes that only three or four nineteenth-century American historians made a significant contribution to the cartography of the Revolution, and Petchenik says much the same of those working in the twentieth century. She believes that in the past seventy-five years historians have increasingly preferred analysis to narrative and that, as a result, they have neglected military history in general and cartography in particular. With the exception of an Elroy McKendree Avery or a Lester J. Cappon, historians have been unwilling to take the trouble to provide accurate, informative, and attractive maps; to seek the help of professional cartographers; and to exploit technological innovations in printing.

This is an important collection of essays, one that will annoy many historians of the Revolutionary War, but one that all should read. Harley is clearly right in urging that maps be classified and studied as they were used. But it remains to be seen whether even the most thorough reordering of cartographic data will improve our understanding of particular decisions—for example, why Howe went to Frog's Neck in 1776 or to the Chesapeake in 1777. Similarly, Petchenik is right in saying that many historians have taken far less trouble with maps than with interpretations. But it is likely that more historians than she allows do take cartography seriously and do try to make maps integral, reliable, and attractive parts of their books. It is also likely that, however skilled they become, cartographers will continue to fall short of perfection in rendering eighteenth-century landscapes and in capturing the complexities of a prolonged and sprawling war. In short, these essays are valuable because they show that historians and cartographers of the Revolution still have a great deal to learn from each other.

IRA D. GRUBER
Rice University

NOBLE E. CUNNINGHAM, JR., editor. *Circular Letters of Congressmen to Their Constituents, 1789-1829*. Volume 1, *First Congress-Ninth Congress, 1789-1807*; Volume 2, *Tenth Congress-Fourteenth Congress, 1807-1817*; Volume 3, *Fifteenth Congress-Twentieth Congress, 1817-1829*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture. 1978. Pp. lxiii, 517; vii, 521-1,012; vii, 1,015-1,634. \$75.00 the set.

Since 1791, United States congressmen have been communicating with their constituents through circular letters. These three volumes make readily accessible for the first time 269 circular letters, a nearly complete set for the first twenty Congresses (1789-1829). Considered by John Adams to be the best single source for the history of the early American republic, these documents have hitherto been widely scattered in depositories and newspaper files.

The editing of this collection by Noble E. Cunningham, Jr., is simply superlative. The introduction is an important contribution that mines the letters to expand our knowledge of parties and politics. A directory of congressmen briefly identifies the 120 legislators who distributed circular letters, and more than 75 pages of index enhance their usefulness. Editorial notes follow each letter and are detailed enough to provide essential background or identifying information without being burdensome. Because the notes to the first letters reprinted for each Congress are useful for understanding later letters, the editors have not repeated them, but the system of cross referencing is a bit inconvenient because, instead of citing the page for the cross reference, the editors cite the author and date of the letter of the cross-referenced note.

Circular letters in the early republic usually were prepared toward the end of a congressional session, rather than near election time. They summarized events selectively; foreign affairs tended to dominate. Congressmen from five Southern states authored almost 85 percent of the circulars. Cunningham explains the regional concentration as the result of low newspaper circulation, the geographical remoteness of voters, weak party organization, and the incidence of electing congressmen by districts rather than at large. The persistence of a gentry-style of politics in much of the South may have impelled Southern congressmen more than others to keep in personal touch with influential local leaders on whose support they depended. Virginia's Samuel J. Cabell, for example, explained in his March 28, 1800 circular that his "letters are merely intended to afford information to those who have honoured me with their representative trust, and not for the public in general," and he requested that "those gentlemen to whom

they are addressed, will not consider them for publication" (p. 181). Letters were detailed, dry, and usually intended for "informed constituents," Cunningham says. Yet practice varied, for 21 percent of the circulars appeared only in newspapers and probably reached a wider audience than the mailed circulars. Southern Republicans were somewhat more likely to distribute circulars than were Southern Federalists: 70 percent of the congressmen from Virginia and the Carolinas were Republicans in the Fourth through Seventh Congresses and account for 81 percent of the circulars sent from those states. The incidence of circulars fluctuated greatly over time. Twenty-one circulars appeared during the first four Congresses. The Fifth and Sixth Congresses saw an explosion, producing thirty-one circulars. The fluctuations are puzzling. The intensity of party rivalry does not adequately account for variations. The Congress meeting from 1811 to 1813 produced only six; the next Congress (1813-15), twenty-three. The last three Congresses included here (1823-29) account for just over 20 percent. There is, in fact, no discernible trend from the Fifth Congress.

The circulars deserve close scrutiny by students of national party development and local politics in the South. Their contents duplicate the partisan press, but they bear, too, a congressman's own imprint. They form, in short, a major addition to the published sources for the political history of the period. Finally, if New Jersey's congressman Ephraim Bateman spoke truly in 1819, he cast fresh light on a hitherto shrouded subject: "Wishing to be entirely frank, on every subject I have introduced, I feel it incumbent to state," he wrote, "that there were a few members of the congress of the United States, (to the honor of the American electors but few) whose libidinous characters and dissipated habits were disgraceful to themselves, and a reproach to the respectable body to which they belonged" (p. 1,077).

PAUL GOODMAN
University of California,
Davis

NOBLE E. CUNNINGHAM, JR. *The Process of Government under Jefferson*. Princeton: Princeton University Press. 1978. Pp. xii, 357. \$20.00.

Soon after he took office President Thomas Jefferson complained to James Monroe that "the expences of this government were chiefly in jobs not seen; agencies in every part of the earth, and for the most useless and mischievous purposes, and all of these opening doors for fraud and embezzlement. . . . They have been covered up from the public under the head of contingencies, quartermaster's department, etc." (p. 22). *Plus ça change,*

plus c'est la même chose. Jefferson, as he often did, was exaggerating, but this study of *The Process of Government under Jefferson* reveals that federal institutions and practices are older than we think. Then, as now, Congress relied heavily on the executive branch to furnish information that was needed to draw up legislation since Congress had no staff of its own. And dinners at the executive mansion were not only social affairs but also occasions for gently twisting congressional arms.

Noble E. Cunningham, Jr., has presented Jefferson's presidency, not as a narrative history, but as a study in how government functioned in the early republic. To do so he has used especially the very important—and neglected—records of the executive departments and the two houses of Congress. Eight chapters are devoted to an analysis of the executive branch, revealing how the president and his colleagues made decisions on the patronage, the budget, foreign affairs, and the delicate, complex business of cooperating with the Congress.

He then studies Congress itself with special attention to the functioning of the committee system and the admixture of party politics in the legislative process. This latter might take the form of pressure from the executive, adjurations from state political leaders, or petitions from constituents. Although congressmen recognized that petitions were not barometers of popular opinion, they did represent pressure groups that were sometimes just as formidable as public sentiment, and Cunningham believes that their influence has been neglected by most historians.

Cunningham is convincing in his assertion that Jefferson was a much more capable administrator than his predecessors. He kept the reins of government more firmly in hand than either Washington or Adams. Moreover the President and his men worked hard (this was also generally true of congressmen). An eight-hour day was usual and the press of business often found department heads and their staffs at work in the evening and on weekends. Most historians have treated the office of attorney general as a sinecure but it was not under Jefferson. That officer was not only the legal consultant for the executive branch, but also his opinion was frequently sought by congressional committees, and he participated fully in cabinet decisions. All three attorneys general shared John Breckinridge's complaint that "I have really not a moment that I could call my own" (p. 137).

It is doubtful if things ran quite so smoothly in Jefferson's government as Cunningham would have us believe. It might have been useful if he had dealt more fully with some of the discordant episodes: the violent squabble between Secretary of the Treasury Albert Gallatin and Robert Smith over the handling of naval funds, which provoked a

congressional investigation; or the breakdown in the enforcement of the embargo at the end of the second term.

But these are minor matters compared with the overall impact of Cunningham's work. He has opened up a wholly new area of investigation into sources known to exist but hitherto simply overlooked or neglected. He has revealed vast sins of omission by historians who have ignored the role of executive departments and Congressional committees—and the attendant pressures exerted on them—that were critical in the shaping of national policy.

This study will undoubtedly lead to the rewriting of a great deal of the history of the years when the Jeffersonian Republicans were in power.

JOHN S. PANCAKE
University of Alabama

SAMUEL B. FORTENBAUGH, JR. *In Order to Form a More Perfect Union: An Inquiry into the Origins of a College*. Schenectady, N.Y.: Union College Press. 1978. Pp. xiv, 130. \$10.00.

Samuel B. Fortenbaugh, Jr., distinguished attorney, business executive, and for many years chairman of the board of Union College, has written a rich, elegant, unpretentious work on the origins of his alma mater. Based on rare manuscripts as well as the latest monographs, his study includes important bits of information on early New York, higher education, and American culture.

Union College was the product of disparate individuals and groups—George Clinton, the populist, and Robert R. Livingston, the patroon; Abraham Yates, the arch-Antifederalist, and Philip Schuyler, the ultra-Federalist; the Dutch Reform Church in competition with the Presbyterians; Albany against Schenectady. Underlying the occasional conflict of the founders was a fundamental consensus on the need for higher education in the young republic. The name they chose was appropriate—"Union."

The pedagogic values of the founders are explored in Fortenbaugh's most interesting chapter on the origin of the Union motto and seal. The seal, adopted in 1795, shows Minerva, wearing a liberty cap, encircled by the motto: "Sous les lois de Minerve nous devenons tours frères." Fortenbaugh traces the motto to Fénelon's *Télémaque*, "one of the seminal works in the canon of eighteenth-century liberalism" (p. 86). Ability to translate *Télémaque* (rather than Greek) was one of Union's entrance requirements. And a graduate of 1804 later recalled that it was studied in the classroom "to teach the principles of civil rule, the characteristics of wise and wholesome laws, and

the correct spirit of administration, as also the more universal and abiding rule of moral and religious life" (p. 88). Obviously, the work had a profound impact on Union and its founders.

It is surprising, therefore, to find that Fénelon and *Télémaque* are not even mentioned in the standard texts on the Enlightenment in America. Though Fortenbaugh warns in his preface (p. xiv) that "a historian is not likely to be interested in this specialized study," I hope that he is wrong. There is much to be gained from this non-academic's institutional history.

STEVEN J. NOVAK
University of California,
Los Angeles

C. A. WESLAGER. *The Delaware Indian Westward Migration: With the Texts of Two Manuscripts (1821-22) Responding to General Lewis Cass's Inquiries About Lenape Culture and Language*. Wallingford, Pa.: The Middle Atlantic Press. 1978. Pp. xiii, 266. \$16.00.

For anyone interested in a brief account of the migration of the Delaware Indians from their ancestral home on the eastern seaboard to their present location in Oklahoma, this is the book. Indeed, the spare style of the narrative was adopted to serve as a frame of reference for two manuscripts that are transcribed in the work. C. A. Weslager admits from the outset that for the migration account he borrowed from his more comprehensive *Delaware Indians, A History*. The final portion of the book quickly recounts the completion of the Delaware trek.

The description of the Delaware removals across Pennsylvania, Ohio, Indiana, Missouri, Kansas, and finally into Oklahoma covers about eighty pages and is not footnoted, but the context is generally familiar. Two transcribed, but unannotated, manuscripts fill the center portion of the work. For the average reader this material will hold little interest. The first item is a questionnaire circulated among Indians of the Old Northwest in 1821 by Lewis Cass when he was governor of Michigan Territory. Weslager has pieced the questions and answers together from separate documents now deposited in the Burton Historical Collection at the Detroit Public Library. The questions cover a wide range of ethnographic material. One can only wish that Cass had been a more thorough anthropologist and, even more, that his assistants had known what they were about. Cass's queries are certainly superior to the answers he received, and he was generally disappointed with the quality of the information. To improve on the questionnaire and to make further inquiries, Cass employed a young man, Charles C. Trowbridge. The Cass-

Trowbridge manuscript comprises the second transcription. Perhaps Weslager should have used his knowledge of the tribe in annotating questionable or obscure passages in the documents, thus giving a better perspective on the data. Nonetheless, this raw material could serve a variety of scholars in linguistics, sociology, anthropology, and history. In the final section Weslager offers an interesting sketch of the Delawares' successful suits against the federal government to regain losses which they had incurred over the years through forced removals. Here Weslager, who served as tribal consultant in the litigation, provides some fascinating detail.

The book is not documented, but footnoting is not actually required in the method Weslager employed. Each section, however, does contain a list of selected references. The maps are helpful and well spaced throughout the work, and the index is useful. Libraries and students of Delaware Indian history will want this book; others may desire a broader account of the tribe.

GARY E. MOULTON

Southwestern Oklahoma State University

ANTHONY F. C. WALLACE. *Rockdale: The Growth of an American Village in the Early Industrial Revolution*. New York: Alfred A. Knopf. 1978. Pp. xx, 353. \$17.50.

Rockdale joins a group of recently published monographs treating the connections between technological and social change in early industrial America. The setting is a cluster of seven textile mill hamlets—collectively called “Rockdale”—in southeastern Pennsylvania. The objective is to delineate the “paradigmatic processes of culture change” by assessing the new machine technology and relating it to an “organized structure of conflict” that existed between “well-intentioned men and women all striving toward a better age” (p. xvi). The result is an absorbing account of how managers, workers, and their families initiated and responded to the new world of factory production.

Of the four parts of the book, the first two are particularly impressive. Based on a variety of primary sources, they provide solid documentation on the social structure of the community, the basic innovations that transformed it into a textile center, and the extensive network of business and family relations that fashioned these changes. The third section addresses the problems of acculturation that accompanied the process of industrialization. The author maintains that the essential question was “who *should* control the machines of the Industrial Revolution” (p. 244). Here he de-

votes considerable attention to elaborating the controversies that spanned several decades and existed at two interrelated levels. Among the local elite, conservative evangelical Protestants vied with a few freethinking infidels in an ideological struggle over the nature of government, property ownership, and moral conduct in the young republic. At the same time the utilitarian imperatives of mechanization elicited a concerted effort by mill owners and manufacturers to eradicate deviant behavior among the laboring population through the establishment of such agencies of social control as churches, Sunday schools, and rigid work rules. By the early 1840s, the force of this two-pronged “evangelical counterattack” had successfully overcome the influence of radical freethinking at Rockdale. Moreover, the outlines of a new industrial creed that subtly incorporated evangelical and Enlightenment ideas had begun to emerge. Called “Christian capitalism,” the creed stressed social harmony rather than conflict and, abetted by a series of unsuccessful strikes and a lingering depression, “worked to redefine the cotton mill operative as a semi-skilled laborer, not a craftsman, politically passive, with conventional social views and a limited capacity to organize in opposition to the owners and managers of the mills” (p. 388). Having reduced the working class to a state of dependency, the leading families established a “moral dominion” over Rockdale that subsequently shaped the community’s reaction to the seething political issues of the 1850s. The final section fleshes out this scenario, showing how Rockdalers directed their millennial enthusiasm toward the politics of tariffs, slavery, foreign missions, and, ultimately, the Civil War.

What makes *Rockdale* so valuable is not so much its general interpretation as the number of separate insights that reflect the author’s ethno-historical perspective and cast light on the internal dynamics of the community. Of special interest are his observations on settlement patterns and how they reflected Rockdale’s physical and social stratification. Equally suggestive are his comments on the contrasting yet complementary styles of family organization among workers and manufacturers and how lines of patronymic descent among the latter group became the basis for forming powerful family alliances. Indeed, the telling effect of these extended kinship networks is forcefully demonstrated by the author’s examination of the role of upper-class women, who formed a closely knit “sisterhood” within the region and, through various benevolent activities, served as the “custodians of social virtue.” The sense of mutual obligation that pervaded the managerial class, the qualities of the Christian gentleman, the leveling effects of mechanization, the relationship between the polit-

ical issue of free land and worker morale, the symbolism of mechanics institutes in splitting the "mechanicians fraternity into two unequal classes" (p. 232)—these and other subjects give the volume a distinctive quality not often encountered in the historical literature.

The particular strengths and weaknesses of this book depend on the way it is read. On the one hand, it succeeds remarkably well as a colorful and anecdotal local history narrative. On the other, it falls short of providing the sort of analytical rigor one might expect of such a study. Particularly disappointing is the author's failure to relate Rockdale's experience to his paradigmatic model of cultural change. One wonders, for instance, if in part three Wallace had stood back from the subject and asked "what does all this mean?" whether he would have been satisfied with the evangelical versus infidel theme in explicating the fundamental cultural conflicts that pervaded Rockdale during the 1830s and 1840s. Moreover, as much as he seeks to reconstruct Chester Creek's working-class culture, the very nature of his sources (many of which are culled from the du Pont family correspondence) prevent him from relating its history "from the bottom up." In this respect, his extended discussion of the benevolent qualities of Christian capitalism will doubtlessly draw criticism from those who view the institution as an exploitative force. So too will his ingenious, if controversial, interpretation of millennialism as the controlling feature of social action during the 1850s. Clearly Wallace has an emotional as well as an intellectual attachment to Rockdale, and, in his effort to recover lost information and tell the "whole story" of the community, he often gets immersed in a welter of detail that tends to cloud rather than clarify the main issues. Two such examples are his extended discussions of utopian socialism (p. 275ff.) and of "Hattie's Boys" (p. 467ff.). In short, the book could have profited from fewer diversions and a tighter organization.

Despite these caveats, *Rockdale* is a very important study. Few books have explored the social dimensions of technological change in greater depth and fewer still have advanced such provocative hypotheses. Readers are bound to be impressed not only by the massive amount of fresh research but also by the volume's flowing style, superb illustrations, and overall impact.

MERRITT ROE SMITH
Massachusetts Institute of Technology

RONALD G. WALTERS. *American Reformers, 1815-1860*. (American Century Series.) New York: Hill and Wang. 1978. Pp. xiv, 235. \$10.95.

For better and for worse, this book is a model of its kind: the brief history written for general readers on the basis of secondary sources. Writing in that genre is risky. The danger is incorporating defects as well as assets of the existing literature. Where the defects are those of theory—as they are in monographs on antebellum reform—the result can be, as this book is, a peculiar combination of historical vigor and intellectual anemia.

The body of *American Reformers* is nine chapters treating many of the more important or notorious reform and radical movements of the day: Protestant evangelicalism, religious and secular communitarianism and utopianism, abolition, women's rights, pacifism, temperance, efforts to induce people to remodel their bodies and their minds, workingmen's movements, and institutional reforms for the poor, the insane, criminals, and the uneducated. Ronald G. Walters does an excellent job with all of them. He understands the secondary literature. He slices deftly to the core of each movement he discusses and compresses masses of material into lucid and attractive forms. He understands the ambivalence of many reformers about political action: they thought politics a knaves' game, but believed that success demanded political power. More important, Walters recognizes that the spirit of evangelical Protestantism—individualism, voluntarism, escaping from the bondage of evil and sin—pervaded the reform impulse, even in its resolutely secular manifestations. Better yet, Walters knows (although he does not explain it as fully as he might) that the diversity of reforms expressed the common effort of hopeful and yet troubled people to deal with an awesomely changing society, yet only in terms of what their culture and history permitted.

But *American Reformers* is weak because it contains no adequate theory of reform. Unlike most of his colleagues Walters grapples with the problem of definition, but he comes off second best. For him reformers are "those who wish to improve individuals or existing social, economic, and political arrangements" (p. xii). From a generous viewpoint this includes practically everybody. But Walters is not generous, and so the reformers he thinks worth considering are those who participated in a reform movement. That is, "any collective, organized effort to improve society or individuals by achieving some well-articulated goal" (p. xiv). Now all sorts of movements that Walters omits fit that definition: Jacksonian and Polkian Democracy; the Whiggeries of Adams and Clay; town and city booster organizations; and many more. So does nativism, which Walters refuses to treat because its adherents were allegedly "fuzzy about what sort of social order they wanted, once they purged America of sinister conspiracies" (p. 12). But the

purge was a "well-articulated goal," and so was Protestant control of the nation.

Until historians have both an adequate definition of reform and the will to follow wherever it leads, they cannot adequately understand reform phenomena. It is good for general readers to have summaries of the state of the art. But thirty years after Alice Felt Tyler's *Freedom's Ferment*, it is time for the art to move on.

CLIFFORD S. GRIFFIN
University of Kansas

ELLEN CAROL DUBOIS. *Feminism and Suffrage: The Emergence of an Independent Women's Movement in America, 1848-1869*. Ithaca: Cornell University Press. 1978. Pp. 220. \$12.50.

"I have written this study because I am a feminist and a radical," explains the author in her introduction (p. 20). The current women's liberation movement "raised the historical questions my book is intended to answer" (p. 10). Not until women could overcome their dependent status and break through the barriers to participation in public life, maintains Ellen Carol DuBois, could they achieve liberation. Thus the woman suffrage movement was not merely "an isolated institutional reform" but rather a "social movement, reflecting women's aspirations for and progress toward radical change in their lives" (p. 17). One of the chief antisuffrage arguments was the assertion that women's interests were represented by their voting husbands or fathers. The demand for female suffrage "challenged the idea that women's interests were identical or even compatible with men's." Suffragism "embodied a vision of female self-determination" and enfranchisement "involved a radical change in women's status, their emergence into public life" (p. 46).

DuBois concisely summarizes the emergence of suffragism out of the abolitionist movement in the 1840s. Although Garrisonian abolitionism provided suffragists with a platform and an ideology linking women's plight with that of the slaves, DuBois argues that "the development of feminism before the war was restrained by the organizational connection of its leaders with the antislavery movement, which kept them from concentrating on the mobilization of women around a primary commitment to their own rights" (p. 19). Thus the split in the suffragist ranks in 1869, instead of weakening the movement as most historians have maintained, actually strengthened it by freeing its most militant faction from the incubus of abolitionist and male ascendancy.

This split occurred over the question of priorities for Negro suffrage and woman suffrage. Most

male abolitionists and Republicans wanted to strike first for black enfranchisement while the iron of Reconstruction was hot. Many women abolitionists agreed. But Susan B. Anthony and Elizabeth Cady Stanton led a group of suffragists who urged a constitutional amendment to enfranchise both blacks and women. Embittered by Republican opposition, they even turned temporarily to the racist Democrats in their search for allies. After discovering that the few apparently sympathetic Democrats were only playing at suffragism to embarrass the Republicans, Stanton and Anthony turned next to labor reformers for support. But they discovered that even working-class women were concerned more with bread and butter issues than with the franchise. Frustrated, the militant suffragists decided that "women themselves must do the work." In 1869 Stanton and Anthony formed the National Woman Suffrage Association. Most male and many female suffragists refused to join this organization and formed instead the rival American Woman Suffrage Association. For twenty-one years the feminist movement remained split. DuBois's sympathies are clearly with the Stanton/Anthony faction. She recognizes the class and racial bias that crept into its opposition to the enfranchisement of "ignorant" blacks and immigrants while "intelligent" women remained voteless. But she believes that this faction "significantly advanced the movement, liberated it from its subservience to abolitionism . . . and opened the way for woman suffrage to develop into an organized movement of women, ultimately one of mass proportions" (p. 201). The importance of this achievement outweighed any of the movement's shortcomings.

DuBois's feminist perspective enables her to bring the movement of a century ago to life in trenchant prose. But this perspective can also be a handicap, for it tends to exclude other perspectives. The abolitionists and Republicans who insisted that to unite the unpopular issues of female and black suffrage would ensure the defeat of both were probably right. DuBois does not confront this question. Nevertheless, for anyone interested in the early history of American feminism, this book is essential reading.

JAMES M. MCPHERSON
Princeton University

PAULA BLANCHARD. *Margaret Fuller: From Transcendentalism to Revolution*. (Radcliffe Biography Series.) New York: Delacorte Press/Seymour Lawrence. 1978. Pp. xii, 370. \$11.95.

Paula Blanchard's *Margaret Fuller* is eminently successful in accomplishing one of its author's in-

tentions, to "contribute to a deeper understanding both of why Margaret Fuller became the kind of woman she was and why the public image of her became so distorted" (p. 1). A new biography was needed to address just these problems. This book, published in the Radcliffe Biography Series on "lives of extraordinary American women," will do a great deal to dispel the ignorance of those women (and men too) who "still have never heard of her." Further, it will correct the mistaken impressions of those still believing the "Margaret Myth" that she was a presumptuous and arrogant bluestocking full of pretensions and delusions.

Some other of Blanchard's objectives and assumptions help to account for the excellence of her book. She thinks that insufficient attention has been paid to the dramatic change that took place in education when Fuller was about ten or eleven years old: a decided shift away from a rigorous, intellectual curriculum, to a new emphasis on domestic and maternal virtues. Fuller's career as a woman writer of serious literary criticism and as a female intellectual was definitely against the tide of the times, resulting in isolation and derision. Her life bridged "the gap between Transcendentalism and social activism" (p. 5). Blanchard's emphasis on the "level-headed" and "classical" side of Fuller's character is a welcome balance to the more familiar romantic who was emotionally unstable.

The volume is filled with the names of persons and places and other specific details, yet there is an easy gracefulness in style. Fuller is often allowed to speak for herself through an abundance of quotes from letters and journals. Careful documentation and a useful bibliography confirm the conviction that this biography was well researched. It brings its subject to life so well because the author makes such good use of all the available materials. Only highly specialized literary studies have been omitted. The general reader as well as the specialist can use this new biography with pleasure and confidence in its accuracy.

Margaret Fuller is here in all her complexity and versatility, from her early teaching and "Conversations," through the Transcendentalist years, friendship with Ralph Waldo Emerson, and authorship of *Woman in the Nineteenth Century* to the time she spent as a book reviewer for Horace Greeley's New York *Herald Tribune* and then her period of political activism as a sympathetic observer and hospital attendant during the ill-fated Roman Revolution of 1848-49. Blanchard notes that Fuller's manuscript on the revolution was lost in the shipwreck that cost the lives of Margaret Fuller, her husband, and their son. Blanchard fails to mention, however, that authentic notes about the revolution taken from surviving journals have been

published by Leona Rostenberg in "Margaret Fuller's Roman Diary," *Journal of Modern History*, 12 (June 1940): 209-20.

By placing a remark or an action in context, all the details and the insights serve to make Margaret Fuller's seeming absurdities and eccentricities more understandable. Her grandiloquent reply to Carlyle, "I accept the Universe," is properly understood as an expression of "a profound acceptance of everything in her life, both good and evil, as part of a divine plan" (p. 86). There is a fair-minded analysis of the relationship between Fuller and Hawthorne which recognizes his dislike of strong, intellectual women as well as her occasional tactlessness and overbearing qualities. There were also times when each took pleasure in the other's company.

What Margaret Fuller *was* as an inspiring and shaping influence may be even more important than what she *did*, although her accomplishments are noteworthy. "She shares with Poe the distinction of being our first major literary critic; she was our first woman foreign correspondent; she contributed much to the feminist movement and to the rise of American Romanticism. She went further than any other woman of her time in forcing an unwilling public to accept the idea of a woman as a major intellectual figure" (p. 342).

RUSSELL E. DURNING
Northern Illinois University

GERALD N. GROB. *Edward Jarvis and the Medical World of Nineteenth-Century America*. Knoxville: University of Tennessee Press. 1978. Pp. ix, 300. \$14.00.

By using a Massachusetts physician, Edward Jarvis (1803-1884), as an example, Gerald N. Grob argues that mid-nineteenth-century American medicine, before the germ theory of disease, functioned in two ways. Doctors treated patients, mostly without great effect. But many physicians also believed that their profession required that they try to influence both individual lives and public policy. These activities in education and philanthropy made early Victorian medicine, therefore, the ancestor—and to some extent the functional equivalent—of modern social sciences.

Jarvis was a well-trained but unsuccessful practitioner. His personal peculiarities drove patients away and deprived him of mental hospital appointments for which he was particularly qualified after he became interested in psychiatric problems. When he was forty, Jarvis began to take a small number of mental patients into his home, and from then on he prospered. He was important, however, not because of his practice but because of his writings and his activities as a social reformer.

Through his books and other publications, Jarvis pioneered in advising school children and other Americans to follow the laws of health, a nineteenth-century combination of personal hygiene and morality. His abiding faith in the efficacy of circumstance and habit in improving health led him into leadership in both the sanitary (public health) movement and the gathering of social statistics, which he believed would provide all of the information necessary for relevant social policy making. He contributed greatly to the upgrading of the federal census, and through his report on mental illness in Massachusetts (1855), a landmark in epidemiology, he helped bring social categories such as class and ethnic group into medical and public policy discussions.

Jarvis was not personally colorful or interesting. Grob has, therefore, written an intellectual history, and in the style of that kind of history the author rationalizes each new set of facts, so much so that at times he strays into the area of speculation. Otherwise the work is a model of documentation, based upon rich archival sources as well as a thorough understanding of the institutions and events in which Jarvis was involved. Although understandable to a general reader, the book will appeal mostly to specialists.

JOHN C. BURNHAM
Ohio State University

JAMES C. BONNER. *Milledgeville: Georgia's Antebellum Capital*. Athens: University of Georgia Press. 1978. Pp. xii, 307. \$14.50.

Milledgeville was the fourth capital of Georgia. Savannah, the first seat of government, was "strongly affected by Toryism" (p. 1) during the Revolutionary War, and backcountry legislators succeeded in moving the capital to Augusta in 1786, to Louisville in 1795, and to Milledgeville, in 1807. Named for Governor John Milledge, 1802-1806, the new seat of government was located near the geographical center of the state and was also situated at the head of navigation on the Oconee River.

The town did not prosper. As of 1860 its population was only 2,230, the smallest of any capital in the Southern states except Tallahassee. One problem was transportation. Navigation on the Oconee proved inadequate, the roads were terrible, and when railroads came to Georgia in the 1830s, Milledgeville found itself off the main lines. The Gothic statehouse (said to be the first public building in the United States of that style) was unimpressive, but the Greek revival executive mansion was a handsome building. The constant threat that the capital would be moved to Macon or to

some other western Georgia town prevented the state government from improving its public buildings, and the statehouse grounds presented an unsightly appearance.

As of 1860, however, Milledgeville appeared to be solving its problems. A new railroad and a telegraph line helped bring an end to isolation, and a referendum in 1855 seemed to have put the removal question to rest. Furthermore, the town did not suffer extensive damage during the Civil War, and the public buildings were improved to some extent during the years of presidential Reconstruction.

Disaster struck with the advent of congressional Reconstruction, when General John Pope, partly in response to rumors that Milledgeville innkeepers would not accommodate Negro politicians, directed the upcoming constitutional convention to meet in Atlanta. This action, combined with the devious tactics of Atlanta "boosters," resulted in removal in 1868.

James C. Bonner, author of several works on Georgia history, is well qualified to write this volume. Using a wide variety of sources, he presents a detailed account of political, economic, and social developments in Milledgeville to the time of removal. Limited space is devoted to events since 1870, because, as the author states, there have been no significant or unusual developments in the history of Milledgeville since that date.

Those interested in local history will welcome this study. Others, less familiar with Georgia history, might wish for a page or two on the long era when Savannah was the capital. A map showing the railroads of the 1850s and the location of Milledgeville in relation to Savannah, Augusta, Macon, and Atlanta would be useful to readers unfamiliar with Georgia geography. This reviewer would have welcomed some comparison with the removal of capitals in South Carolina, North Carolina, and other states on the Atlantic coast. Nevertheless, this book well accomplishes the objectives of the author and the readers for whom it was intended.

DANIEL W. HOLLIS
University of South Carolina

THOMAS L. WEBBER. *Deep Like the Rivers: Education in the Slave Quarter Community, 1831-1865*. New York: W. W. Norton. 1978. Pp. xiii, 339. \$14.95.

Langston Hughes was eighteen years of age and riding aboard a train that was crossing the Mississippi River at sunset when he wrote his most famous poem, "I've Known Rivers." Hughes was in a pensive mood that day and had begun to think about the history of black people, their African

heritage, and their experiences in America. He thought, too, of rivers that were a part of the African-American's past: the Congo, the Niger, and the Nile. But he pondered most about the muddy Mississippi and what that river had meant to blacks—how being sold down river must have been a traumatic experience for the slave. As Hughes reflected, he felt proud of the efforts of his people to maintain their dignity under the harshest conditions America could offer, and he quickly completed the last stanza of his poem, ending it: "My soul has grown deep like the rivers." It is quite appropriate that Thomas L. Webber, in his examination of the slaves' struggle, takes the title for this book, *Deep like the Rivers*, from Hughes's epic poem.

Webber is interested in what slaves understood and felt about their world and how they acquired their understanding and feelings. He argues that white teaching on the plantation was aimed at making the blacks into obedient, trustworthy, hardworking slaves. A primary operating principle on most plantations was to keep their chattel as ignorant of the outside world as possible. Allowing the slaves to celebrate holidays was a way of pacifying them. Religion was used to imbue the slaves with those values and attitudes that the slaveowner deemed appropriate. But despite these efforts, Webber concludes, most blacks rejected the master's teaching. The quarter community, according to him, was a society within a society, with its own values, attitudes, ideas, and "educational instruments" (p. 250).

The chief problem with Webber's argument, however, is that he does not persuasively demonstrate that "most"—a term he employs far too often—slaves were successful in rejecting the slaveowner's teaching. The slave narratives are helpful but it must be kept in mind that most slaves did not leave an autobiography or have their experiences recorded. Webber's failure to consider this more perceptively leads him to paint a picture of plantation slaves living in relative pleasure with their nuclear and extended families, children at play, slave women treated considerately by the master, nurseries provided for slave infants, and accommodations that afforded the slaves "crude comfort" (p. 8). In short, he leaves the reader with the impression that the peculiar institution really was not that bad.

This reviewer is not attempting to suggest that Thomas L. Webber supports the contentions of Ulrich B. Phillips or that the slave narratives should not be used. There is value in Webber's book. He attempts to document further the wholeness, humanity, and dignity that the African-American maintained despite enslavement. The personal recollections of the slaves should and

must be utilized, and it can be done effectively and with discernment, as such scholars as John W. Blassingame, Herbert G. Gutman, and Leslie Howard Owens have demonstrated.

DONALD SPIVEY
University of Michigan,
Ann Arbor

WILLIAM F. MESSNER. *Freedmen and the Ideology of Free Labor: Louisiana, 1862-1865*. (University of Southwestern Louisiana History Series, number 12.) Lafayette: University of Southwestern Louisiana. 1978. Pp. xiii, 206. \$6.00.

A major problem confronting Union military officials in occupied Louisiana during the Civil War was management of a sizable black population that was no longer willing to be enslaved but not yet really free. Despite individual philosophical differences, the men who headed the Gulf Department essentially forged their freedmen's policies to meet the needs of economic recovery, racial stability, military strength, and preparation of the former slaves for future participation in a free-labor society. Blacks in occupied southern Louisiana were deployed mainly in two ways: as agricultural workers for wages or shares on confiscated or "loyal" plantations under the aegis of military provost marshals or Treasury Department officials and as soldiers in such Federal units as the Native Guard and later the Corps d'Afrique. Also initiated were work-farms or "home colonies" for aged and infirm freedmen and a surprisingly successful network of schools for black soldiers and children. These programs, characterized by a combination of bigotry, pragmatism, and true idealism on the part of white authorities and exploitation and severe hardship, yet hope and a shred of human dignity for Louisiana blacks, are thoroughly explored in William F. Messner's *Freedmen and the Ideology of Free Labor: Louisiana, 1862-1865*.

Messner deserves commendation for meticulous research, an ability to reconstruct complex events with clarity and subtlety, and a genuine commitment to intellectual integrity. His perceptive analysis of the constant political maneuvering among Unionist military and civilian leaders in wartime Louisiana is outstanding, as are his descriptions of noncombatant military life and Gulf Department educational endeavors (especially among soldiers). Especially praiseworthy is his evenhanded treatment of the sensitive subject of racial prejudice on the part of white officials in the Unionist hierarchy; Messner has portrayed saints and sinners on the race question without once descending to the currently common practice of imposing mid-twentieth-century standards on nineteenth-century men.

If this volume possesses one major shortcoming, it is Messner's reluctance to weave his study into a broader historical context. Little effort has been made to relate these wartime innovations to the racial and economic realities of Louisiana's antebellum past or to future developments in the state during Reconstruction. He has made a limited attempt to relate the Gulf Department programs to occupational experiments being tried concurrently elsewhere in the South, but more elaboration would have been helpful, especially on the critical topic of land redistribution. Messner has chosen instead to limit his focus largely to thirteen sugar parishes in southern Louisiana during three crisis years. Within these narrow parameters, however, he has fashioned an outstanding monograph worthy of serious consideration.

ROGER A. FISCHER
University of Minnesota,
Duluth

LOREN SCHWENINGER. *James T. Rapier and Reconstruction*. (Negro American Biographies and Autobiographies.) Chicago: University of Chicago Press. 1978. Pp. xx. 248. \$22.00.

Loren Schweninger assumes two tasks in his biography of James T. Rapier: to correct the factual errors that abound in various Rapier biographies and "to examine the sweeping social, economic, and political developments during the nineteenth century through the mind of an intelligent, sensitive, dedicated, and astute black leader" (p. xix). The first task he does very well indeed; the second is more problematic. Schweninger's impressive and wide-ranging search (from Canada to New Orleans) into the antebellum records gives us a fuller portrait of Rapier's family background and youthful influences than we have of any other black Reconstruction leader. Rapier's personal correspondence, however, is thinnest during the most critical period of his life—the years of his political initiation and congressional service—and it is precisely at this juncture that Schweninger is forced to rely heavily on the very sources he condemns for not providing a "black perspective," that is, white newspapers and personal correspondence. Consequently, although *James T. Rapier and Reconstruction* is one of the better contributions to the growing biographical literature on black Reconstructionists, it is inadequate as a treatment of Alabama Reconstruction or even of the black perspective on Alabama Reconstruction.

Correcting previous accounts, Schweninger describes Rapier's origins, not as the son of a white planter, but of a well-to-do free mulatto barber

who was the twelfth-wealthiest Negro in Alabama in 1860. The Rapier Papers at Howard University are richest on this fascinating free Negro family whose members demonstrated an incredible talent for business enterprise and speculation. Schweninger traces Rapier's career from his education in Canada to his activities as a political organizer in Alabama to his term in Congress to his struggle for political patronage in the post-Reconstruction era. Yet, one puts down this book so rich with detail on Rapier's life feeling one still does not know the man. Part of the problem may be that, as so often happens in biography, the author has grown so uncritically fond of his subject he tends to overstate Rapier's role, his unselfishness, and his idealism.

Factionalism within the Alabama Republican party is a major theme in Schweninger's study. He convincingly sustains the thesis that the native Republican factions' refusal to accept their black colleagues as political equals doomed Reconstruction. Nevertheless, the factions never come entirely into focus, and one is not confident that the basis for their competition has been fully explored. Schweninger emphasizes conflict between native and northern Republicans, but it is doubtful that this alone accounted for the schisms, especially in light of Horace Mann Bond's seminal discussion almost forty years ago of Alabama's nascent capitalists and their political influence.

Rapier's relationship to these factions is not adequately explored. Indeed, the source, content, and consequences of Rapier's conservatism (Schweninger calls it "moderation," "magnanimity," or lack of "extremism") is never clarified. Schweninger repeatedly and uncritically cites the praise Rapier received from conservative editors and politicians. But these he presents as evidence of Rapier's integrity, respectability, and character, rather than questioning why white conservatives felt more comfortable with Rapier as the lesser of evils among his black colleagues.

Finally, Schweninger does yeoman's work in searching out the deeds, mortgages, and other business papers that reveal Rapier's extensive speculations in Tennessee and Alabama; he rented plantations, hired tenants, and earned huge profits. It may be that Rapier was as altruistic a planter as he was a politician, but one cannot decide on the basis of what we are told here. As his father did before him and Booker T. Washington did later, Rapier commended education and thrift to his freed brethren. Given his background, such bourgeois values are not surprising; the author's lack of comment on them is.

THOMAS HOLT
Harvard University

HOWARD N. RABINOWITZ. *Race Relations in the Urban South, 1865-1890*. (The Urban Life in America Series.) New York: Oxford University Press. 1978. Pp. xxii, 441. \$17.95.

In this study Howard N. Rabinowitz treats race relations in Richmond, Raleigh, Atlanta, Montgomery, and Nashville during the generation following emancipation. With freedom, blacks joined whites in migrating to the cities, forming more than half of the population in Montgomery and Raleigh and only slightly less than half in Richmond, Atlanta, and Nashville. The great majority found employment as domestics and unskilled laborers. Massed at the bottom of the economy, they occupied the worst housing, suffered the poorest health, and filled the prisons. Furthermore, they were thoroughly segregated. In the manuscript census returns for Atlanta, Richmond, and Montgomery, the author found "ample evidence of substantial early segregation which seemed to increase from decade to decade," a finding affirmed for Raleigh, Nashville, and other cities both by his impressions and by the work of other scholars (p. 105). He concludes that from 1865 to 1890 "Southern urban centers were divided into separate black and white worlds" (p. 226).

Specifically, physical segregation was the rule in housing, churches, schools, hotels, restaurants, bars, theaters, parks, zoos, fairs, brothels, orphanages, insane asylums, hospitals, poorhouses, prisons, cemeteries, athletics, and entertainments. The highest degree of mixing occurred in public accommodations. Blacks in the cities were particularly effective in utilizing the streetcars. Between cities, however, they were usually relegated to second-class cars on the railroads, and steamboats were the most segregated of public conveyances. Even where there was some degree of proximity between whites and blacks in public accommodations, lines were nevertheless drawn by such devices as screens and partitions and by separate entrances, service windows, seating areas, water closets, and exits. Thus, even in this sphere of maximum mixing, "*de facto* segregation generally prevailed" (p. 182).

"Segregation was neither a tactic invented by the Redeemers to punish blacks nor the result of a bargain between Democrats and Populists in the 1890s to forestall Negroes from becoming a pivotal force in Southern politics," Rabinowitz asserts. Rather, "Northerners in the form of the U.S. Army, the Freedmen's Bureau, and Republican politicians together with their Southern allies inaugurated much of the segregation" (p. 127). The alternative to segregation was not integration but exclusion. If blacks were to have access to schools,

hospitals, and orphanages, these facilities would have to be segregated. White Republicans, seldom free of prejudice themselves, supported separate and equal facilities as a way of attracting white voters while rewarding their black followers. Blacks accepted separation in pursuit of their prime objective, "equal access." Moreover, they began to organize their world as one apart, centering their communities tightly around church and school. Radical Reconstruction seldom breached the color line, and the Redeemers' policy was to "continue already existing segregation" (p. 187).

In politics black people went from exclusion to exclusion. In none of the five cities did black voters ever control politics. White politicians wanted Negro votes, not Negro officeholders. But in the end, the problem for whites was not that black people voted the wrong way but that they voted at all. When the North signaled that it was safe to do so, the South disfranchised black men. By 1890 Southerners were spurred to disfranchisement and the enactment of Jim Crow statutes generally by the assertiveness of a generation of young Negroes reared after emancipation.

There is a massive labor of primary research behind this volume. The author has thoroughly worked the basic sources for five important Southern cities. He offers scholars a wealth of factual information, well organized and smoothly presented. Moreover, the book sparkles with insights and bristles with interpretations that are independent, provocative, and bold. To say, for instance, that during Reconstruction the North and the Republicans initiated most Southern segregation will raise hackles that will not be soothed by the qualification that the alternative was exclusion. To assert that black people accepted segregation will undoubtedly draw fire that will not be abated by the assertion that separate and equal facilities were better than no facilities at all. And in the perennial debate on the origins of segregation, Rabinowitz is emphatically on the side of those who see a widespread and relatively rigid *de facto* segregation in place early in Reconstruction.

This work impressively affirms a picture of the origins and evolution of the physical color line in the South that had been building for a generation. Massive *de facto* segregation came close upon the heels of emancipation. It was never total, even after waves of segregation legislation had washed over the South. And in relatively narrow areas integration was achieved after emancipation and persisted for periods of varying lengths. There was fluidity, but the fluid ran uniformly toward segregation and exclusion, not integration.

Now that we have drawn the history of the physical color line in the South with reasonable clarity,

perhaps it is time to focus our attention more centrally upon the question of *why* there was a line. Inevitably we must look closely again at the mind of the South, white and black, and attempt to understand better the racism that was the root cause of the segregation, disfranchisement, and proscription of black people in the South for a century after emancipation. And if Rabinowitz is correct that Northerners were the prime movers behind "much of the segregation" in the South, we must look also and yet again at the mind of the North as it has contemplated the black presence in America.

JOEL WILLIAMSON
*University of North Carolina,
Chapel Hill*

WILLIAM M. ARMSTRONG. *E. L. Godkin: A Biography*. Albany: State University of New York Press. 1978. Pp. xix, 287. \$30.00.

The author of *E. L. Godkin and American Foreign Policy 1865-1900* (1957) and editor of *Gilded Age Letters of E. L. Godkin* (1974) has now produced a full-length examination of the career of the genteel editor of the *Nation* and *Evening Post*. The longevity of William M. Armstrong's research on Godkin notwithstanding, this biography will disappoint scholars seeking fresh insights into the ideas or thorough analysis of the editorial opinions of one of the mightiest pens of the late nineteenth century.

Much of the book is a warmed-over version of Armstrong's earlier works. Large passages have been lifted—often verbatim—from the 1957 volume, but only once does Armstrong trouble to cite his own book. The chapter, "Founding the *Nation*," reproduces almost literally Armstrong's March 1967 article in the *Journal of American History*, "The Freedmen's Movement and the Founding of the *Nation*." This article is not cited, nor does it even appear on the list of Armstrong's own publications in the bibliography. The biography (and the earlier works) are based on exhaustive foraging into manuscript and journalistic sources. There seems, however, scant attention to recent secondary literature. For example, Armstrong's discussion of Godkin's liberalism might have been improved by consulting Robert Kelley, *The Transatlantic Persuasion* (1969). His rendition of the Liberal Republican movement might have utilized Matthew T. Downey's "Horace Greeley and the Politicians: The Liberal Republican Convention in 1872," which ironically appeared in the very issue of the *Journal of American History* as Armstrong's article.

The derivative nature of Armstrong's work

ought not to obscure consideration of his findings. In contrast to "Mugwump historians" from Rollo Ogden to Allan Nevins, his portrayal of Godkin is unsympathetic. Brusque in dealings with those beneath his social status, unscrupulous in reaching for journalistic power, a gloomy elitist as the century waned, the editor was a narrow conservative who lacked "the intellectual outlook the times demanded." He made the *Nation* "a repository for the conventional wisdom of a minority of the 'best people' . . . a repository for their prejudices" (p. 135). Similarly, he challenges as "uninformed" the view that Godkin was an anti-imperialist. While opposing annexation of Hawaii and the Philippines on racial grounds, Godkin nonetheless supported economic exploitation of those regions. In his hands, indeed, the *Evening Post* became the organ of capitalist interests of the country. He makes a case that Godkin's view on western development foreshadowed Frederick Jackson Turner's frontier thesis. His chapter describing the relationships among Godkin, Carl Schurz, and Horace White and Schurz's eventual ouster as editor-in-chief of the *Evening Post* in favor of Godkin reflects a sensitivity to the personal characteristics of each man.

The book, however, lacks a systematic analysis of the ideological underpinnings of the *Nation's* countless editorial positions. Armstrong comes to grips with only a handful of issues on which Godkin fulminated over a generation, and his eclectic approach does not provide sufficient clarity. English Liberalism, which he identifies as Godkin's intellectual wellspring, is too complex to be confined within phrases such as "the laws of trade." Thus, while this volume is a useful synthesis, the standard intellectual portrait of Godkin remains unwritten.

RICHARD ALLAN GERBER
*Herbert H. Lehman College,
City University of New York*

THOMAS A. BRYSON. *Walter George Smith*. Washington: Catholic University of America Press; distributed by International Scholarly Book Services, Forest Grove, Ore. 1977. Pp. xiii, 225. \$13.95.

Walter George Smith's public career covered the years from 1877 when he became a practicing lawyer in Philadelphia until his death in 1923. An astute attorney, a Catholic, and a Democrat, Smith won a niche for himself in the dominantly Episcopal and Republican elite of Philadelphia by forging connections with the city's first families through law partnerships and marriage to a Drexel. Active in the movement to upgrade his

profession's ethical and educational standards, Smith eventually became president of the American Bar Association.

Smith was a figure of either secondary or tertiary importance in a variety of political and humanitarian movements. Like many men of his class, he supported municipal reform of the good government type, participating in several efforts to defeat Philadelphia's well-entrenched Republican machine. In the early 1900s Smith campaigned for passage of stricter divorce codes and uniform state commercial laws. In the 1920s his standing as a lawyer-philanthropist led to his appointment as a member of the United States Board of Indian Commissioners. His major public contribution, however, was in the area of post-World War I relief work, where he became a leading American Armenophile, lobbying at the Paris Peace Conference and in London and Washington for measures to protect the Armenians. Thomas A. Bryson's treatment of this phase of Smith's career comprises the longest and most detailed section of the book and offers a useful description of ways in which economic and strategic considerations prevailed over humanitarian and religious motives in American policy toward the Armenians.

Professor Bryson repeatedly refers to Smith as a progressive, although the label applies to Smith only in certain very limited ways. Smith was a conservative reformer who opposed liberalized divorce laws and women's suffrage and who abandoned the Democratic party to vote for Hughes in 1916 and Harding in 1920 because Wilson had supported the Adamson Act, the Keating-Owen child labor bill, and the income tax provisions of the Underwood Tariff.

Effective and energetic in his professional and humanitarian pursuits, Smith nevertheless met with many setbacks. He failed in two campaigns for elective office and in 1901 figured prominently in an unsuccessful effort to overthrow the Republican machine that controlled Philadelphia city government. His advocacy of tighter divorce laws did not win public favor, and his proposals for protecting the Armenians after World War I were not adopted by the international community. But these defeats did not dominate Smith's career or drive him into retreat from public life. His biography thus exemplifies the life of a particular type of public figure, the civic-minded professional.

GERALD W. MCFARLAND
University of Massachusetts,
Amherst

PAUL AVRICH. *An American Anarchist: The Life of Voltairine de Cleyre*. Princeton: Princeton University Press. 1978. Pp. xxii, 266. \$16.50.

An intellectual who never wrote a book, a radical who was never imprisoned or achieved notoriety, Voltairine de Cleyre (1866-1912) was best known for her saintly advocacy of anarchism. Nevertheless, the standard histories of the movement by Joll, Woodcock, and Guerin do not mention her; nor is she included in *Notable American Women*. To call this first biography definitive may seem like faint praise, but in fact Paul Avrich's book is a sensitive and insightful monograph about a figure whose "writings anticipate the contemporary mood of distrust toward the centralized bureaucratic state. She was one of the most eloquent and consistent critics of unbridled political power, the subjugation of the individual, the dehumanization of labor, and the debasement of culture" (p. xix).

The daughter of a French immigrant who admired Voltaire, de Cleyre was educated in a Michigan convent, eventually extending her distaste for religious authority to hostility to all forms of compulsion. She spent her mature years mostly among Jewish immigrants in Philadelphia, where she wrote articles, pamphlets, fiction, and poetry for the anarchist press and, less occasionally, lectured. Her views were eclectic and ecumenical, grounded in sentimental humanitarianism and religious yearnings rather than in economic analysis. A de-ranked former pupil wounded her severely in 1902, but otherwise de Cleyre's life was uneventful, constricted by poverty and lack of recognition beyond anarchist circles. She was buried in the same Chicago cemetery as the Haymarket martyrs, whose deaths had inspired and haunted her own career of radical agitation.

The idealism that animated her work, the privation and sickness she endured, and the sadness that never eluded her account for her biographer's sympathetic treatment. Unpleasant aspects of her personality—the cold rigidity of her asceticism, the neuroticism of her withdrawal from ordinary human companionship—are noted but minimized. Avrich's claims for de Cleyre's excellence as a thinker and writer are not borne out by his quotations from her work, which in their florid haziness offer temptations to skim which other readers may find hard to resist. Certainly she failed to resolve the fundamental tensions in anarchist thought between individualism and collectivism and between pacifism and violence.

Avrich deserves commendation for making a minor figure interesting, for his thorough command of primary and secondary sources, and for his own prose that combines force and feeling. This contribution to the history of American anarchism—and women—is not huge (commensurate with de Cleyre's importance) but is nonetheless welcome. But if anarchism "was not an alien doctrine, but an integral part of the American past" (p. xiv),

then why were its adherents so few, so unpopular, and so likely to be immigrants? De Cleyre's limited influence suggests the marginality of the movement. Although Avrich eschews psychobiography, de Cleyre's loneliness and pain and her ravaged health and luckless love affairs meant suffering perhaps too intense to be cured by the rearrangement of political and social institutions.

STEPHEN J. WHITFIELD
Brandeis University

JAMES R. GREEN. *Grass-Roots Socialism: Radical Movements in the Southwest, 1895-1943*. Baton Rouge: Louisiana State University Press. 1978. Pp. xxiv, 450. \$24.95.

This book is by far the best local or regional study yet published on American socialism in its heyday before the First World War. In itself this is faint praise, since to this reviewer's knowledge James R. Green's book is only the second such effort to appear, the other being Henry Bedford's valuable but much more limited *Socialism and the Workers in Massachusetts, 1886-1912* (1966).

The bulk of *Grass-Roots Socialism* is taken up with a rich, detailed, and magnificently researched narrative describing the rise, influence, and decline of socialism in rural areas of the Southwest between 1890 and just after the First World War. After the collapse of Populism in the late 1890s, Green shows how, instead of reverting to one or the other of the two major parties, sharecropping tenants, poor white cotton farmers, black workers in the Brotherhood of Timber Workers, and other disaffected groups came together in adjacent areas of northern Louisiana, eastern Texas, and southern Oklahoma to create a genuinely new radical alliance. It gave strong support both to the I.W.W. and to Socialist Party candidate Eugene Debs, whose largest single state vote in 1912 came from this area of the country. In doing this, Green confirms that it is possible, with much diligence and effort, to put together a comprehensive, bottom-up narrative that conveys the full flavor and complexity of local militant action, as well as taking to task other historians—including, quite rightly, this reviewer, on the contribution of United Mine Workers' members to the amalgam—for their earlier concentration on the national trade union scene. Among much else that is new, Green shows us the connection between regional militancy and the syndicalist example of the Mexican revolution; the varying economic, political, and social motives for radicalization; and that violence or direct action was taken up by the Working Class Union at the end of the period not out of ignorance, blind rage, or naiveté, but as a reasoned response to state

oppression and electoral disenfranchisement. This latter achievement enables the author to rescue the antiwar Greencorn Rebellion of August 1917 and other violent episodes from the condescending dismissal which has been dealt them by other historians and to put them instead in their proper confederate, vigilante, and antimilitarist historical context. But Green's most valuable contribution is to show how, if only for a brief period, an authentic, rural socialist-workers alliance could come into being that was neither yeoman-romantic in its ideology, nor solely neo-Populist in its composition. Instead, it was a wide-ranging democratic movement that, had it not been for the First World War, might well have had a profound long-term influence on the politics of the area.

There are some weaknesses in Green's account. He exaggerates when he says that all Wobblies were expelled from the Socialist Party of America as a result of its 1912 antisindicalist ruling (p. 180); when he asserts (p. 341) that Debs was converted to socialism in Woodstock jail after the 1894 Pullman strike; and when he neglects to mention, in his discussion of the 1911-1915 Harriman railroad strike, that IWW dual unionism was one of the important reasons for the strike's failure. He also might have been more analytic and less purely descriptive in various other parts of his narrative. He could have reflected further over what he labels as a "Proletarian Perspective" in his discussion of the Brotherhood of Timber Workers, for example, a phrase that is not entirely satisfactory in describing what he evidently regards as a socialist form of consciousness. But these are minor blemishes. Overall, James Green—not surprisingly, in view of the high quality of his other published work—has written an authoritative, compelling, and innovative study of rank-and-file socialism in the southwest which will long remain the standard work for its regional area. Methodologically speaking, it will serve as a model which all future historians of U.S. socialism will be unable to ignore.

JOHN H. M. LASLETT
University of California,
Los Angeles

BRUNO RAMIREZ. *When Workers Fight: The Politics of Industrial Relations in the Progressive Era, 1898-1916*. (Contributions in Labor History, number 2.) Westport, Conn.: Greenwood Press. 1978. Pp. viii, 241. \$17.50.

This attempt to describe the political economy of labor relations includes chapters on the coal industry, the metal trades, and the unskilled worker in the mass production industries. It also analyzes the economics and technology of industrial pro-

duction, the character of the trade agreement, and the genesis and operation of strike mediation. But no new synthesis emerges from this account, the details of which do not enrich (and rarely match) the finely textured work of Marguerite Green, James Weinstein, and Melvyn Dubofsky.

The author suggests a field for detailed investigation when he provides some evidence that after 1902 increasing competition from foreign manufacturers, especially in Germany, stiffened employer opposition to labor unions. When read in conjunction with the material on American economic growth in chapter five, the chapter on the IWW suggests that American employers were enraged by the IWW not only because it espoused revolution and opposed trade agreements but also because the organizing activity of the IWW came at precisely the point when intensified mechanization was greatly increasing the proportion of factory laborers in the unskilled and semiskilled categories.

In covering familiar ground, the narrative is weakened by the failure to define the "public" that held attitudes on different approaches to labor relations. The extent of the spread of scientific management by 1916 is greatly exaggerated. The slowing of the American economic growth rate between 1907 and 1914 hardly justifies categorizing the period as one of economic "crisis." Many labor historians will feel that the author has slighted the importance of the *de facto* union recognition obtained by the UMWA in the anthracite fields in 1903. Research in the National Civic Federation archives (which the author consulted) suggests that his contention that the leaders of the AFL truly accepted factory welfarism is way off the mark.

This volume's analysis of the attitude of employers toward labor unions would have been enhanced by explicit recognition of the fact that virtually all employers who signed trade agreements were making pragmatic accommodations with unions that they basically did not want to exist. Employers readily abandoned collective bargaining whenever technological advances or political developments undermined the power of worker organizations.

ROBERT ASHER
University of Connecticut

JAMES J. KENNEALLY. *Women and American Trade Unions*. (Monographs in Women's Studies Series.) St. Albans, Vt.: Eden Press Women's Publications. 1978. Pp. ii, 240. \$15.00.

This study is an examination of women and American trade unions from 1865 to the present. Inter-

persing narrative and biographical vignettes. James J. Kenneally explores a number of inter-related themes: the place of women within trade unions, trade unions and feminism, and the interactions of working women, women reformers, and male union leaders. Unfortunately, the author has no explicit thesis or argument and hence no clear basis for the inclusion or exclusion of material. Furthermore, the study, despite the use of a variety of collections of primary sources, is basically derivative. In succeeding chapters, Kenneally offers little that is new in the way of interpretation, and the reader would often do better relying on the secondary literature cited in the notes.

The problems stem primarily from the conceptual framework the author brings to his work. The study is basically what Gerda Lerner and others have called "compensatory history." It seeks to break the "silence of historians" regarding women and to show that women have made significant contributions in American history. Its focus is on the "heroines" among working women and women reformers and on their "determination, courage, and loyalty" (p. ii). Such a perspective, if well done, may make for good propaganda and contribute to consciousness raising, but it makes for rather weak history.

It is weak history because it offers little analysis of the larger forces which impinge on events and on the historical actors. Many important working women and reformers appear on stage in Kenneally's account, and they play major parts in the labor struggles of New York City garment workers, Lawrence textile operatives, and Cripple Creek miners. But everything appears in unsubtle brush strokes with valiant women struggling against cruel, grasping capitalists aided by the power and authority of the state. Explanations of success and failure are invariably couched in personal terms; in this context the male chauvinism of Samuel Gompers and the humanitarian support of Eleanor Roosevelt take on a greater importance than they may in fact have had. To understand the broad trends discussed in this study we need a better understanding of the structural forces that shaped events. We need to consider forces influencing the responses of ordinary working men and women and not just to examine the actions and attitudes of leaders thoughtful enough to leave voluminous collections of papers for future historians.

Women's history has experienced a heartening growth in the past decade both in terms of sheer quantity and in its conceptual sophistication. Unfortunately, Kenneally appears to be largely uninfluenced by these developments and offers little more than a compendium of events, organizations, and individuals in the history of women and trade

unions in the past century. We are past the stage where such an enumeration is particularly helpful; only when these facts are placed within a broader conceptual framework and interpreted accordingly shall we have a piece of scholarship likely to make an important contribution.

THOMAS DUBLIN
University of California,
San Diego

JOHN HIGHAM, editor. *Ethnic Leadership in America*. (Johns Hopkins Symposia in Comparative History, number 9.) Baltimore: Johns Hopkins University Press. 1978. Pp. x, 214. \$12.95.

These essays, the outcome of a symposium at Johns Hopkins University in 1976, break new ground in exploring ethnic leadership in the United States. Editor John Higham establishes the keynote: if ethnic boundaries weaken, ethnic groups in an open society become in some measure the creation of their leaders. In an afterword anthropologist Sidney Mintz suggests that ethnic leadership may depend upon the relationship of ethnic values to those of the wider society.

Higham's introduction, an extraordinarily perceptive overview of the forms of ethnic leadership, is followed by Nathan Glazer's analysis of the transition from internal conflict that characterized Jewish leadership in the era of mass immigration to the emergence of agreement on one dominant issue, the survival of Israel. For the Japanese, external forces were fundamental; as Roger Daniels demonstrates, the Japanese and American governments created situations requiring community leadership, as in the case of the Japanese American Citizens League. For the heterogeneous German Americans, ethnicity was an inadequate bond: Frederick Luebke brilliantly reveals how bitter disillusionment during World War I was followed by twenty years of divided, ineffective leadership. Afro-American leaders such as Booker T. Washington are labeled by Nathan Huggins as "emblematic" to differentiate them from "reformers" who attacked racial caste. After World War II they were replaced by new leadership relying increasingly on a black power base. American Indian leaders, as Robert Berkhofer shows, have also been accommodators or protesters. Diverse tribal histories and fragmentation of authority complicate the current problems of both intra-tribal and pan-Indian leadership.

Josef Barton focuses upon voluntary associations of Czechs, Slovaks, Rumanians, Slovenes, Croats, and Italians, whose leaders had been prepared by their homeland experiences to transcend traditional loyalties of family and neighborhood.

In Slovak and Italian communities, other leaders with little ideological attachment and often only marginally identified with the community made demands for power and mobility. In contrast, Czech, Slovene, Croatian, and Rumanian communities each achieved a continuity of local and national leadership through federations that adjusted to a changing society yet also maintained ethnic leadership.

The psychic quality and the functions of Irish leadership are delineated by Robert Cross, who sees the proliferation of Irish American leaders as deriving from a "combination of exuberant optimism and mordant pessimism." (p. 195). Loyalty was a crucial element in Irish leadership in politics and the church. Largely because priests made themselves personally available and effectively upheld their superiors' authority, the Irish gained control of the Roman Catholic Church in the United States.

A brief review cannot convey the nuances of these mostly analytical essays. They vary so widely in substance, viewpoint, and style that Higham's preliminary observations are indispensable. After reading the volume this reviewer returned to the introduction and found it even more rewarding.

In a book that will be widely read, it is painful to see so many typographical errors and misspellings, disproportionately so in Huggins's essay (for example, "Niagra Movement," p. 102; "Plessy v. Fergusson," p. 111; "torturous," "intractibleness," "influencial," and "bourgeois," pp. 105, 106, 107, and 110).

ROBERT ERNST
Adelphi University

JOHN I. KOLEHMAINEN. *From Lake Erie's Shores to the Mahoning and Monongahela Valleys: A History of the Finns in Ohio, Western Pennsylvania and West Virginia*. Ohio Finnish-American Historical Society. 1977. Pp. vii, 355.

After researching for more than forty years on Finnish immigrants in America, John I. Kolehmainen has revised his doctoral dissertation on Ohio Finns, incorporating substantial portions of eight of the dissertation's nine chapters into his book. His revisions involved copying or recasting long passages and abridging the discussion of particular organizations, as well as introducing information on Ohio Finns since the 1930s and adding sections on Finnish immigrants in West Virginia and Western Pennsylvania. Besides inserting two extracts by other writers, the author also reproduced or summarized material extensively from his other writings. In addition, he included fifty-six photographs illustrating mainly organizational

life. The result was this book of eleven chapters which dispenses with footnotes and instead provides bibliographical suggestions.

The book emphasizes the history of the Ohio Finns from the 1870s to the 1930s. Reflecting the detailed research of Kolehmainen's dissertation, it describes the settlement and employment of Finns in Ohio communities like Ashtabula Harbor. Its major chapters also examine and catalogue the strife-ridden activities of churches, temperance societies, and workingmen's associations. Furthermore, the book updates these chapters showing how organizational activities declined after immigrant numbers fell from a peak of 6,406 in 1920 to a low of 1,209 by 1970.

In contrast, the experiences of Finns in West Virginia and Western Pennsylvania are treated as afterthoughts to the chapters dealing with their Ohio counterparts. Brief summaries catalogue the growth of settlements as well as the establishment of temperance and church activities in the two states. The labor movement receives less attention. This treatment of the two states, like that of Ohio, says little about the effects of the Great Depression and World War II and ignores the impact of anti-radicalism after 1945.

Besides tracing the evolution of Finnish-American communities in the three states, the book synthesizes a veteran historian's scholarship on the Finns. It places the author's case study in the context of his understanding of Finnish experiences in America generally. Its chapters begin with overviews drawn from his writings. They also introduce new themes, such as home life and immigrant relations with Finland, that were not covered by his dissertation. The book is informed, moreover, with a theory of Americanization emphasizing the adoption of the English language, the rejection of radicalism, and the acceptance of Finns by Americans. In summary, Kolehmainen's synthesis is a personal and somewhat nostalgic tribute to the disappearing immigrant community of his parents.

A. WILLIAM HOGLUND
University of Connecticut

LLOYD P. GARTNER. *History of the Jews of Cleveland*. Cleveland: Western Reserve Historical Society and Jewish Theological Seminary of America. 1978. Pp. xv, 385. \$15.00.

Cities are central to Jewish development within Western civilization. Cultural diversity is recalled in Spanish Toledo's "Golden Age," in Frankfurt, or in Warsaw. Few cities claim such eminence within America and surely Lloyd P. Gartner does not for Cleveland. Yet, the demographic shift to

the New World since the 1800s is undeniable, along with an increasingly crucial role in Jewish survival. Gartner's impressive account of communal development contains an essential validity far beyond local history. Internal growth and dynamic leadership did, indeed, project Cleveland into prominence in American Jewish life. But the underlying historic processes, inevitably, required also that analysis of broader events so amply demonstrated throughout this work.

Thus, the hazards and opportunities of America's urban frontier emerge, akin to Bayrd Still's classic Milwaukee study. Cleveland had but 6,000 people in 1840, including Irish, German, and Jewish immigrants. Yet, even earlier, Jews appeared in the public mind, as in the *Cleveland* [sic] *Register's* 1819 reproof of Maryland for refusing to grant them full rights. Such issues foreshadowed the arena of intergroup attitudes whether in the Civil War, Great Depression, or World War II.

Here, also, is unfolded the relative pluralism of the American Jewish community. Gartner evokes meaning and clarity. Thus, there is no doubt of Cleveland's Jewish pioneers hailing from Bavaria, nor of the human balance sheet producing their "America fever." Yet, aside from an Alsbacher or Thorman, we read also of the Joachimsens from Breslau and of the Sephardic Peixottos. And even before the "new immigration" swamped the established community, East Europeans initiated their own congregations and schools.

Sectarian creativity and cooperative ventures produced new synagogues and service institutions, though often thwarted by dissidence. Cleveland's location in the heartland of Reform Judaism, as well as influential rabbinic and lay leadership, inspired recurrent controversy and change well into the twentieth century. Among such shifts, eventually, was an enhanced support for Zionism in which Cleveland's Rabbi Abba Hillel Silver played a major national role. Gartner's treatment of Silver within the context of American and communal concerns reflects a judicious attitude so essential to ethnic history replete with "personalities." One can only wish for more work on relations between America and Israel (this book ends at 1945, with an enlightening afterword by Moshe Davis).

Gartner's incisive analysis and scholarly documentation create a significant communal, social, and political history. Still, he does not lose sight of individuals, including those many who never attained positions of leadership or wealth. For the American dream ever encompassed socioeconomic self-advancement, even if unrealized.

Somehow, the countless definitions of success—whether through Cleveland's flourishing industries, in professions, politics, or Hebrew poetry—

are effectively woven here into a definable Jewish community. In the process, Gartner clarifies the uniqueness and meaning of American experience.

JOSEPH BRANDES
William Paterson College

RAYMOND L. HALL. *Black Separatism in the United States*. Hanover, N.H.: University Press of New England, for Dartmouth College. 1978. Pp. x, 306. \$15.00.

The author of this interesting commentary on black nationalist and separatist behavior from 1787 to the early 1970s says that "there is a paucity of serious works on black separatism in the United States" (p. vii). Curious, for this book is based almost exclusively on secondary sources. The study was conducted between 1968 and 1972 and published in January 1978. Attempts to update it were unsystematic, and significant developments in scholarship and American life were ignored. This has diminished the value of what still could have been a "timely book." Raymond L. Hall attacks his subject with enthusiasm, however, and does provide a lively synthesis of published work on civil rights and black power organizations during the late 1960s and early 1970s.

Part one, which covers the historical background, is weakened by Hall's tendency to repeat his points rather than demonstrate them. One example is his frequent overstatement of the continuity between Booker T. Washington and Marcus Garvey. Citing Harold Cruse, *The Crisis of the Negro Intellectual* (1967), as his source, Hall stretches Cruse's point to assert without further documentation that Garvey came to the United States "at Washington's behest" (p. 242). True, Garvey wrote to Washington, announcing that he hoped to visit Tuskegee, and Washington responded hospitably, but there is no evidence that he intended to encourage anything other than Garvey's early scheme to establish a trade school in Jamaica. Since Hall has based his treatment of Washington on secondary sources, he should have perused Louis Harlan, *Booker T. Washington* (1972). Instead, he depends on publications over twenty years old. He is similarly cavalier regarding recent work on Garvey, "the star" (p. 56) of his study.

Hall is not the first sociologist to approach the history of black separatism. Alphonso Pinkney's *Red, Black, and Green* (1976) is superior and more current; it addresses artistic and literary questions to which Hall seems oblivious. Like Pinkney, Hall avoids statistics and blurs the distinction between black separatism and black nationalism. The subject of marital separatism is neglected, although it might have provided a challenge to Hall, who asserts, "a separatist is always a nationalist" (p. 3).

Part two, which emphasizes the late 1960s, is imaginative and informative, although misleadingly referred to as "The Contemporary Period." Here too there is a puzzling neglect of readily available information. Why does Hall question the sincerity of the Black Muslims' recent integrationism? (p. 162). It was apparently taken seriously by those whites who joined and by the die-hard separatists who withdrew in horror.

Part three, which deals provocatively with the question of social change, is marred by similar difficulties. Hall provides little analysis of the eclipse of militant separatism during the mid 1970s. His analysis of the black studies movement ignores the literature surveyed by Pinkney; it seems based entirely on personal experiences at Dartmouth, and a bias against non-technical education. Strange attitude for a practitioner of soft sociology in a liberal arts college.

WILSON JEREMIAH MOSES
Southern Methodist University

ROGER D. SIMON. *The City-Building Process: Housing and Services in New Milwaukee Neighborhoods, 1880-1910*. (Transactions of the American Philosophical Society, number 68, part 5.) Philadelphia: The Society. 1978. Pp. 64. Paper \$8.00.

The City-Building Process is an excellent case study of the relationship between physical and social transformation in the urban environment. Focusing on the development of Milwaukee's outer city, Roger D. Simon analyzes the interaction of geographic, economic, and social factors in shaping the city's land-use patterns. Three different kinds of communities form the basis for the study: the "Northwest Corner" (Wards 20 and 22) which was largely middle-class and heavily German; Ward 14, which contained mostly working-class families, many of Polish extraction; and Ward 18, in which the city's elite neighborhoods were located.

The significance of Simon's work rests in part on its confirmation of certain constants in the urban-building process of the late nineteenth and early twentieth centuries. For example, increased ethnic and class segregation played a major role in the development of the neighborhoods of Milwaukee—a medium-sized, newer, and comparatively homogeneous city—in a manner similar to the way in which such segregation shaped early twentieth-century Boston or Philadelphia. The determinative role of the subdivider in creating new neighborhoods is also affirmed.

More important, however, is Simon's detailed analysis of patterns which have heretofore not been given sufficient examination by urban historians. First, his stress on the significance of family life-cycle stage as a major determinant of housing

needs provides an expanded perspective on the development of the outer city. Second, Simon demonstrates, rather than simply asserting, the overwhelming importance of home ownership for working-class families. He argues effectively that such ownership, and the economic security which it represented, outweighed even considerations of convenience and public health.

Finally, Simon's work is important as a contribution to the historical analysis of the American suburbanization process. We need to know in what ways the patterns that have been observed in the old Northeastern cities also hold true in other parts of the country. *The City-Building Process* enriches and extends our understanding of those patterns.

MARGARET S. MARSH
Stockton State College

RONALD L. NUMBERS. *Almost Persuaded: American Physicians and Compulsory Health Insurance, 1912-1920*. Baltimore: Johns Hopkins University Press. 1978. Pp. xii, 158. \$10.00.

The medical profession's part in the compulsory health insurance crusade of the World War I era has been described by others but has its fullest treatment in this volume. Ronald L. Numbers attributes the rise of physicians' interest in health insurance partially to financial anxieties growing out of a surplus of physicians in some localities, competition from irregular doctors, and the expanding role of government in health affairs. He observes that general practitioners saw the family-physician relationship passing and watched their poor patients turn to dispensaries, those seeking immunity from contagious diseases to public health doctors, and the rich to specialists.

Real or exaggerated grievances made the profession susceptible to the appeals of the American Association for Labor Legislation, which launched its state-level crusade for compulsory health insurance in 1912. According to Numbers, some doctors supported the movement because they thought it was irresistible and that the profession's interest required their cooperation, while others found the prospect of higher incomes under health insurance most appealing. Some medical leaders cited the inadequacy of current medical care and maintained that a compulsory health insurance system would improve the quality and availability of medical service. Several state societies established committees to study the issue, and the American Medical Association created an influential Committee on Social Insurance.

There were several "rounds" in the compulsory health insurance fight. The first came in 1916 when the AALL introduced bills in three state legisla-

tures and secured endorsement of the insurance principle by two state medical societies. Round two started early in 1917 and had practically ended by April when the United States entered the war; this round reached its conclusion before the AMA's House of Delegates stipulated conditions that any compulsory system must meet. Early in the second round some local societies in New York and a few in other states expressed strong opposition to compulsory health insurance while most societies remained uninvolved and apathetic.

The final rounds began early in 1919 and reached their high point on April 10, when the New York Senate passed a compulsory insurance bill which, by this time, nearly all doctors opposed. Numbers attributes the defection of physicians in New York and elsewhere largely to their rising income during the war and to their growing belief that the movement could be defeated. The chapter describing the profession's contribution to postwar hysteria in New York is probably his best.

The author does not show that any sizable part of the profession was "almost persuaded" to accept compulsory health insurance. He fails to portray adequately the physicians' world of the second decade, largely ignoring, for instance, the havoc wrought among medical societies by the alarming growth of contract practice, and he gives little attention to the "intermediate agent" or "middleman," identifying lay organizations created to provide physicians' services at discount prices and drain off profits. Opponents of health insurance scared countless doctors with the charge that "government medicine" would extend the dreadful evils of contract practice. Little attention is given to the actual abuses physicians suffered under workmen's compensation schemes which increased their fears of government intrusion in medicine. Yet despite simplistic answers to complex questions and inadequate research in some areas, this book throws additional light on numerous points of the health insurance struggle.

JAMES G. BURROW
Abilene Christian University

RONALD SPECTOR. *Professors of War: The Naval War College and the Development of the Naval Profession*. Newport, R.I.: Naval War College Press. 1977. Pp. viii, 185. \$2.75.

This is a thoughtful and carefully researched study of the development of the Naval War College that adds to our understanding of the process of bureaucratization. In the innovative atmosphere of the 1880s, a handful of key naval leaders, impressed by various claims that the "scientific laws" of physics, biology, anthropology, or economics were now discernible, sought to reveal the "laws"

of "sea power" and naval strategy. Using history as a data base, the "open classroom" as a teaching technique, and war gaming as a validating mechanism, men like Luce, Mahan, and William McCarty Little created a lively center for the study of strategy and policy, the U.S. Naval War College.

The College had its tribulations—bureaus and bureaucrats were troubled by its independent existence or dubious of its value. By 1900 it had overcome these problems. A more serious one, however, hastened its ultimate decline: both the civilian Navy Secretaries and Admiral Dewey's General Board persistently directed the War College to leave questions of national (or even naval) policy to others and to stick to "strictly military considerations." Hence, while the War College played a substantial role in the decision to create a navy "general staff" (the Office of the Chief of Naval Operations), by World War I it had become little more than a training institution for mid-level officers in tactics and theater strategy. Mahan's post on naval history remained unfilled after his departure; his lectures became entrenched truth. By the 1920s instruction was "by the book." Instructors paid little heed to submarines or aircraft carriers, and students refought the Battle of Jutland for twenty years. An innovative force had crystallized into a conservative institution.

This is Ronald Spector's story, and he tells it well. I found intriguing the evidence that Admiral Luce wrote pseudonymous letters to the *Army and Navy Journal*, critical of essays of his proposing a Naval War College that had appeared in that journal, in order to draw attention to the proposal. Particularly impressive is Spector's use of the several relevant archives to show that in 1901 the War College staff had "successfully predicted the German strategy two years before the Germans themselves had actually thought of it" (p. 104). I found only one minor typographical error in this well-illustrated and nicely bound publication, one of a new series of historical publications produced by the Naval War College. The author and the publisher merit our appreciation.

PETER KARSTEN
University of Pittsburgh

THOMAS GARDEN BARNES. *Hastings College of the Law: The First Century*. San Francisco: Hastings College of the Law Press. 1978. Pp. xiv, 457.

California, more than any other state, witnessed a proliferation of alternatives to the model of legal education represented in the twentieth century by university law schools like Harvard or Michigan. It is astonishing to find that today of the sixty-five

American law schools not approved by the American Bar Association, forty-eight are located in California (p. 318). These schools claim to operate in the finest democratic tradition by providing opportunities for the poor and academically under-prepared. The legal training they advertise, in contrast to the academic curriculum taught by career law professors to full-time students in the major university law schools, is intensely practical and taught, often at night, by practitioners to part-time students. While such schools have a reputation today, in most cases deservedly so, as merely cram schools for the bar examination, they are the inheritors of a respectable tradition that in the early twentieth century was tremendously influential in terms of the sheer numbers of lawyers produced.

The Hastings College of Law, founded in 1878 as the "Law Department of the University of California" (its actual relation to the University was and remains ambiguous), did not begin as such a school. Its first Dean, John Norton Pomeroy, and his protégé and successor, Charles William Slack, created a school whose high standards compared favorably with most law schools in the country. Yet in the arrangement of its curriculum and in the teaching methods adopted, Hastings ignored the educational reforms, centered upon the case method of instruction introduced by Langdell at Harvard, that many university law schools adopted during the 1880s and 1890s. Indeed, Hastings did not firmly enter the mainstream of legal education until after World War II, when Dean David Ellington Snodgrass rebuilt its academic reputation by recruiting talented faculty (termed the '65 Club) from around the nation who had been forced into retirement but whose energies remained vigorous. In the interim, after the resignation of Dean Slack in 1899, the Hastings College of Law acquired the reputation as an inferior law school. That judgment, based upon the school's reliance on part-time faculty who were active practitioners and the fact that its students studied law only part-time while working in law offices, was rendered by representatives of the nation's most prestigious university law schools. It is a judgment that badly needs reassessment.

This study does not provide that reassessment because it focuses too narrowly on the Hastings College of Law. The result is a leisurely, readable, sometimes sprawling and over-written, traditional institutional history. At times, indeed, the book becomes archival history as the author details, year by year, professors hired and courses rearranged. When it does look outward, the book offers sketches of the political, economic, and legal activities of its faculty and alumni. These achievements are impressive, especially in the case of the alumni, yet it is a curious approach that obliquely argues

the worth of the school through the status of its alumni while never confronting the precise nature of the institution that could produce such lawyers and, at the same time, be judged so harshly by the legal establishment.

The thoroughness of the research and the mass of detail presented will make this book valuable to historians of legal education and it should prove attractive to alumni as well. But its narrow focus does not provide a much needed analysis of the shifting place of the Hastings College of Law in California legal education and thus shed light on the twentieth century debate over the merits of academic or practical legal training.

WILLIAM R. JOHNSON
University of Maryland,
Baltimore County

VALENTIN H. RABE. *The Home Base of American China Missions, 1860-1920*. (Harvard East Asian Monographs, number 75.) Cambridge: Harvard University Press. 1978. Pp. x, 299. \$15.00.

In 1931 the Laymen's Inquiry into foreign missions of Protestant churches leveled bitter and scathing criticism against the overseas missionary enterprise. This book's examination of the way in which leaders at home sought to elevate foreign missions into a nationwide crusade is scarcely less devastating. The easy optimism of transforming the world into a Christian community, the readiness to identify the United States as a Christian nation, and the adoption of Madison Avenue fund-raising techniques contributed to a confusion of means and ends and created naive and unrealistic expectations. The results was a costly backlash after World War I.

The author is not a secular cynic who enjoys throwing darts at organized religion; he is a friendly but tough critic. He believes that the church does have a mission, but this has not inhibited him from presenting an analysis that is highly perceptive, marked by careful research and balanced judgment. His readers may not share his patient understanding of human frailty.

The rapid growth of the missionary movement after 1880 gave rise to an elaborate bureaucracy at home base with men of stature such as John R. Mott, Robert E. Speer, Samuel Capen, and J. Campbell White providing the leadership. Mission boards and interdenominational societies, in need of both the money and prestige successful men of business could provide, welcomed participation of laymen and readily made use of their skills in organization, money raising, use of slogans, and clever appeals. One result was management by an elite who, though not lacking in sincer-

ity and goodwill, confused activity, increased funding, advertising, and large attendance at conventions with constructive work.

As the enterprise grew the financial demands mounted so that mission boards were constantly harassed by deficits. Contributions by the small minority of church members who were interested simply did not suffice to carry the burden of a multi-million dollar enterprise. To raise the needed funds two strategies were adopted: convert all church members into supporters and win large donations from the wealthy. Funds available did increase. The money raisers skillfully made their appeals in terms of Christian stewardship and elevated giving to foreign missions almost to a sacrament, but the funds were never adequate for the support of missionary personnel, schools, and hospitals. And boards learned that a churchgoer and his money are not easily parted.

Valentin H. Rabe provides an excellent analysis of missionary recruiting, of the backgrounds of volunteers, and of their training for their chosen profession. In the years prior to World War I the preparation was wholly inadequate. Why young men and women found the call to be missionaries irresistible is well explained in both religious and secular terms.

This is an excellent study which throws as much light on pre-World War I American society as it does on the nature of the promotion of missions at home. The more or less church-centered society of that era gave way to predominant secularism in the 1920s. The change that took place likewise replaced a bumptious nationalism clothed in religion with a somewhat more humble assessment of the future.

PAUL A. VARG
Michigan State University

GEORGE H. DOUGLAS. *H. L. Mencken: Critic of American Life*. Hamden, Conn.: Archon Books. 1978. Pp. 248. \$15.00.

Why do we need another book about Mencken? We still recognize his liberating influence in combating censorship and winning recognition for new authors; we acknowledge the pathbreaking scholarship of *The American Language*; we laugh as he punctures the hides of evangelists and chiropractors. But George H. Douglas argues that we are ignoring Mencken's principal genius, "his ability to combine a superb comic gift with a very serious penchant for social criticism" (p. 40). Mencken gave us, according to Douglas, "what is perhaps the most successful critique of American life that we have had . . ." (p. 18). To prove this contention, Douglas concentrates on Mencken's writings of

the 1920s—the six series of *Prejudices*, the *Notes on Democracy*, and the pieces in *The American Mercury*. He discusses the authors who influenced Mencken, notably Friedrich Nietzsche and William Graham Sumner, and suggests similarities between Mencken's ideas and those of such recent critics as David Riesman and Richard Hofstadter.

Douglas makes a spirited case for the permanent value of Mencken's social criticism, but I remain unconvinced. Discussing the tools of the literary critic, Mencken said: "If the saw won't cut, he seizes a club" (p. 198). And in social criticism, Mencken was even more ruthless. His blows, which often landed below the belt, do more to preserve his memory as a humorist than to establish his credentials as a responsible critic. One may smile when he says that "a good politician under democracy, is quite as unthinkable as an honest burglar" (p. 132). Yet taken seriously, Mencken's witticism only contributes to cynicism. By indiscriminate labeling of groups he disliked as "puritans," "peasants," and "boobs," he contributed more to the vocabulary of the smart set than he did to sober analysis.

Douglas does well to admire Mencken for his courage, his independence, his love of liberty, and his gusto in exposing the shams of his day. But the permanent value of his social criticism is diminished by his boasted contempt for democracy, equality, and social justice. None of these ideals should be sacrosanct; each should be periodically reexamined. But the most valuable social critic uses a scalpel to excise diseased tissue; he does not clobber living flesh with a club.

NELSON MANFRED BLAKE
Syracuse University

JOHN M. ALLSWANG. *The New Deal and American Politics: A Study in Political Change*. (Critical Episodes in American Politics.) New York: John Wiley and Sons. 1978. Pp. xiii, 155. Cloth \$10.95, paper \$6.95.

A common complaint about quantitative history is that all the elaborate numbers, tables, and charts only confirm what we already knew. Confronted by a familiar episode in the past, the historian may turn to the computer for much the same reasons a film-maker relies on "visuals" to enliven a mediocre script. But neither statistical pyrotechnics nor arty camera work can long distract us, for this is, after all, a conventional plot.

At first glance, this criticism seems applicable to John Allswang's computerized study of the Roosevelt coalition based on the voting patterns of three thousand counties. After acknowledging that the seeds of the new Democratic majority were buried

deep in the soil of the prosperous 1920s (that is, the growing political importance of immigrants and urban-dwellers to whom Republicans were either indifferent or hostile), Allswang repeats the story of the New Deal's rise to power: Hoover's failure to cope with the economic crisis, the brilliance of Roosevelt's campaigns in 1932 and 1936, and the ability of the New Deal programs to win the support of the masses. Allswang's data on which groups constituted Roosevelt's coalition contain no surprises. We are shown once again that the New Deal was supported by the poor and the unemployed, blue-collar workers, immigrants and their children (particularly Catholics and Jews), sharecroppers and tenant farmers, blacks, and the South. The opposition came from what was left: businessmen, affluent farmers, old-stock Americans anxious about their declining social prestige, and inhabitants of small towns who disliked the New Deal's affinity for the metropolis.

Yet Allswang is not content simply to follow the script. Once he shifts his focus to the political subplots of the 1930s, his book becomes genuinely interesting and provocative. For example, Allswang argues forcefully that the New Deal did not destroy the power of the urban bosses as Edwin O'Connor intimated in *The Last Hurrah*. Frank Hague of Jersey City, Ed Flynn of the Bronx, and Frank Kelly of Chicago survived by accommodating themselves and their machines to the politics of reform. Roosevelt did reduce the influence of Tammany Hall but only because he had an alternative: Fiorello LaGuardia. Otherwise, FDR worked with what was available, exchanging patronage for votes, thereby increasing the power of the cities at the expense of the states. And even in the states, the political impact of the New Deal varied according to geography, economic considerations, and the strength of the local Democratic organizations. In these sections of the book, Allswang gives us a portrait of political behavior which is valuable and persuasive.

More significantly, Allswang insists that Roosevelt was motivated as much by ideology as by partisanship. Some of FDR's most egregious mistakes (the court-packing plan of 1937 and the effort to "purge" conservative Democratic legislators in 1938) were caused, according to Allswang, by Roosevelt's desire to preserve and expand the New Deal. Allswang is critical of these endeavors; he seems to regard political success as more important than a doomed defense of principles and policies. This is a dubious proposition even on political grounds since Roosevelt had nothing to lose: the New Deal was stalled in Congress, conservatives of both parties were recalcitrant, and the election of more pro-New Dealers might have renewed the

impetus for reform. Nevertheless, when Allswang concentrates on the *mise en scène* of the New Deal, his book becomes original and worth reading.

RICHARD H. PELLIS
University of Texas,
Austin

HAROLD H. MARTIN. *William Berry Hartsfield: Mayor of Atlanta*. Athens: University of Georgia Press. 1978. Pp. xv, 230. \$7.50.

During the quarter century from 1937 to 1961, William B. Hartsfield served as mayor of Atlanta for all but two years (1940-42). Harold H. Martin, a retired Atlanta journalist and a contemporary of Hartsfield, has written a brief biography of the colorful and often volatile mayor.

When Hartsfield was elected in 1937, Atlanta was a small city of thirty-seven square miles burdened with a debt of thirteen million dollars. On his retirement in 1961, Atlanta's corporate limits had expanded to 138 square miles. The city was fiscally sound and the center of a metropolitan area of one million people. Martin chronicles Hartsfield's extraordinary accomplishments: fiscal reform, the development of a major airport, the construction of a dam to ensure a steady water supply, governmental reorganization, and his most impressive achievement, his skillful handling of the racial situation which resulted in the peaceful integration of the city's public schools.

Martin sketches the portrait of a man in love with his city. Hartsfield's administration was free of corruption. A tireless promoter, Hartsfield was a master at public relations. Who could ever forget the spectacular premier of *Gone With The Wind* in 1939? Yet there was a sad note in the mayor's obsession with his city. Absorbed by the affairs of "the great human family of Atlanta," Hartsfield neglected his own family. Only after his retirement in 1961 did he seem to find happiness with his second wife and her young son.

The principal weakness of the book is Martin's failure to analyze the metamorphosis of Hartsfield from a conventional segregationist into the mayor who guided his city along the path to racial peace. What gave him the courage and the common sense to see the futility of resistance to change? The answer eludes Martin.

The author used Hartsfield's personal papers, interviews, and newspapers as his principal sources. Occasional factual errors (for example, Lyndon Johnson telephoning the stricken Hartsfield from the White House in 1970) mar the text. Although by no means a definitive biography, *William Berry Hartsfield* provides a useful beginning for

the study of Mayor Hartsfield and the Atlanta he described as "a city to busy to hate."

JAMES R. SWEENEY
Old Dominion University

THEODORE H. WHITE. *In Search of History: A Personal Adventure*. New York: Harper and Row. 1978. Pp. 561. \$12.95.

Theodore H. White has been one of the most successful journalists of our times. Born in Boston, the son of poor immigrant parents, he went to Harvard on a scholarship and graduated *summa cum laude*. Then, in 1938, having won more scholarships for foreign travel, he set out for China. While still in his early twenties he knew Chiang K'ai-shek and Mao Tse-tung well and considered Chou En-lai a close friend. He became an intimate of General Stilwell and Henry Luce's star reporter in the Far East. His Chinese experiences led to a book, *Thunder Out of China* (1946), which was both a popular and a critical success. (It sold, White reports, more copies than any book on China except those "famous novels," *The Good Earth* and *Oil for the Lamps of China*.)

Next White turned his attention to postwar Europe. From this experience came another successful book, *Fire in the Ashes* (1953). He then wrote two novels, one set in China, the other based on his time as an editor of *Collier's*. Both these novels were book club selections, and they were made into movies.

After Gary Cooper paid \$80,000 for the movie rights to the second novel, *The View from the Fortieth Floor*, White decided to take two years off "to write a book about how a President is made." He would cover the coming 1960 campaign "as a story." The result, of course, was the first of his immensely successful *Making of the President* books, works that changed the way campaigns are reported and indeed the way candidates conduct them. But after four such books, White explains, he wanted to do something "more challenging." This autobiography is the result.

Once more White has produced both a best-seller and a book that will interest serious students of history. His sketches of the important men he has known are vivid, and nearly all are shrewd and well balanced. "Having been tugged too often by friendship and affection for men I have reported," he writes, "I am as wary of friendship with the great as a reformed drunkard of the taste of alcohol." Three men, he says, totally captivated him—Chou, Stilwell, and John F. Kennedy. "I would now behave otherwise," he adds, and immediately goes on to discuss Chou's ruthlessness, his capac-

ity to act "with the delicacy of a cat pouncing on a mouse" (p. 118).

It is therefore surprising to read the pages of *In Search of History* devoted to Kennedy, for they describe a person few knowledgeable persons will recognize. White attributes enormous influence to Kennedy. "He unlatched the door, and through the door marched not only Catholics, but blacks, and Jews, and ethnics, women, youth, academics, newsmen and an entirely new breed of young politicians" (p. 457). He claims the speech that Kennedy delivered during the 1960 campaign on the separation of church and state "ranks with Lincoln's 'House Divided' speech and Bryan's 'Cross of Gold' as one of the great speeches of American political campaigns" (p. 486).

His portrait is, to say the least, overly reverential. Some parts are plainly apologetic. White excuses Kennedy's having once charged White's Harvard mentor, John K. Fairbank, with having been a "Communist influence" on the state department. "You know what a kid congressman is like with no researchers, no staff, nothing," Kennedy told him. Apparently this satisfied White completely. "His remorse was so real I could not press the matter" (p. 470). White explains away Kennedy's womanizing with the extraordinary statement that of all the presidential candidates he has ever met, only Truman, Carter, and Romney had not "at one time or another, on the campaign trail, accepted casual partners" (p. 529).

White has obviously lost all perspective where Kennedy is concerned, which is a shame. It mars what is otherwise an interesting and informative book, the best autobiography of a journalist I have read since the appearance of William Allen White's *Autobiography* in 1946.

If only he could have understood his feeling for Kennedy the way that other White understood his for Theodore Roosevelt. Describing a plane trip with Kennedy, he writes, "That night, he won me" (p. 467). Describing his first serious conversation with Roosevelt, W. A. White wrote in his *Autobiography*: "I had never known such a man as he and never shall again. He overcame me" (p. 297). But William Allen White retained his reporter's ability to judge from a distance where Roosevelt was concerned; Theodore H. White treats Kennedy as though he were a knight in shining armor. But perhaps this is not so surprising after all. It was he who first publicized Jacqueline Kennedy's characterization of the Kennedy years as Camelot.

JOHN A. GARRATY
Columbia University

CHRISTOPHER THORNE. *Allies of a Kind: The United States, Britain and the War against Japan, 1941-1945*.

New York: Oxford University Press. 1978. Pp. xxii, 772. \$29.50.

This is a serious work for serious scholars—the kind of exhaustive, in-depth study which, unfortunately, will probably not sell many copies, but of which author and publisher can be proud. The subtitle is misleading, for this book is much more than a narrow monograph about the war in the Pacific against Japan. Rather it is a perceptive and provocative examination of the broadest political and strategic issues that characterized the Anglo-American entente during World War II, using the Pacific War as the main focus for the discussion. At the same time, it is a major contribution to the international history of East Asia, continuing the skein begun in the author's earlier study of the Manchurian crisis. All this is based upon documents gleaned from archives, public and private, in Australia, Great Britain, the Netherlands, and the United States, as well as the relevant printed materials, primary and secondary. The label definitive is all too frequently tossed about loosely, but in this case it applies. This study has set the bench mark for future scholars.

The book follows a rigid organizational pattern. It is divided into five parts—a graceful overview of the pre-Pearl Harbor setting, from there to the Casablanca Conference, Casablanca to the Cairo-Teheran talks, from Cairo to the second Quebec Conference (September 1944), and thence to the Japanese surrender. Parts two through five are then divided into six or seven chapters; the world setting, an examination of the military aspects of the war against Japan, the China question, Southeast Asia and colonial empires, India, Australasia, and the problem of postwar planning for East Asia. That fixed format results in repetition and a number of "as we shall see" comments, though it has the advantage of permitting each segment of the book to be understood on its own merits. The sections on India and Australasia might strike American readers as peripheral, but that may be a parochial point of view.

Christopher Thorne offers a fresh perspective as he relates Anglo-American military and political strategies. He deftly illustrates how racism and ethnocentrism prevented British and American officials from developing practical and effective policies toward the Asian nations and provides repeated examples to illustrate the suspicious and competitive way in which Britons and Americans regarded each other. But the basic theme is an overarching sense of sadness and disappointment that the United States and Great Britain failed to create a true community of interest and policies in East Asia. "The exclusive and unrestrained partnership which Churchill, for one, desired," (p.

148) did not materialize, and Thorne is displeased, even annoyed, because he sees no realistic reasons for that failure. There are numerous villains, Hornbeck, Stilwell, and Mountbatten, to name a few, while the assumption by American policy makers (occasionally lumped under the rubric "public opinion") that Anglo-American relations were competitive, particularly in regard to "empire" in East and South Asia, reinforced America's go-it-alone inclinations. The main title, *Allies of a Kind*, is a fitting play on words. England and America were not divided by a common language, but by very real envy and distrust. Ironically, the grim pursuit by American leaders of postwar power and security in the Pacific suggests to me that the logical ground for Anglo-American cooperation in the area was joint defense and expansion of formal and informal empire, but traditional anti-colonialism in the United States and British condemnations of American hypocrisy prevented that sort of *realpolitik* arrangement.

It is in China that Thorne finds the greatest gap between British and American perceptions of each other's true motives. Even though Anglo-American interests in China were very close—each sought stability, the elimination of dominating foreign influences such as Japan or Russia, and access to Chinese markets—Anglophobes like Patrick Hurley, and a number of inept and anti-American British diplomats obscured the reality of those convergent interests. Since the United States played the role of senior partner in the alliance, the choice of cooperation in East Asia lay with the Americans. But Roosevelt consistently made good Soviet-American relations his first priority, in East Asia as well as in Europe. That did not guarantee friction between the United States and Great Britain—even Churchill commented privately, following the Yalta Conference, that the West could trust Stalin (p. 498)—but it did relegate Anglo-American relations in Asia to a less important category.

Scholars may dispute some of Thorne's interpretations, certainly I do. Is it accurate to present Britain as more anti-colonialist than the United States? Were British and American interests in China and much of the rest of eastern Asia largely identical, or is the author more aware of Britain's weakness and need to retrench than was the British leadership during World War II? Was Franklin Roosevelt quite as cynical and ungrateful as he comes off in this book? But none of this diminishes the importance of this piece of scholarship. Replete with well-turned phrases and amusing yet relevant anecdotes, the book is a gold mine of lecture material. More important, no scholar of the World War II era can afford not to read it.

One closing complaint. For whatever reason, the

publisher has chosen to use a flimsy binding (mine fell apart within a week) and paper which makes a reality of the expression "paper thin." That is a disservice to both the author and the audience.

WARREN F. KIMBALL
Rutgers University,
Newark

JAMES J. DOUGHERTY. *The Politics of Wartime Aid: American Economic Assistance to France and French Northwest Africa, 1940-1946*. (Contributions in American History, number 71.) Westport, Conn.: Greenwood Press. 1978. Pp. x, 264. \$17.50.

Most studies of Franco-American diplomacy during World War II have focused on the strained relations between the United States and the French National Committee and on the volatile personal relationship between Roosevelt and De Gaulle. James J. Dougherty's thoroughly researched monograph on American wartime economic aid to France is thus a valuable addition to the literature. Exploiting the vast records of the Foreign Economic Administration and numerous manuscript collections, Dougherty carefully traces the evolution of various American programs of aid to France: the early attempts to send relief supplies to Vichy France; the larger and more important relief programs for French North and West Africa; and post-D-Day efforts to provide relief and rehabilitation assistance for liberated France.

"The story of American economic assistance to France and French Northwest Africa . . .," Dougherty concludes, "was one of frustration, controversy, and limited success" (p. 4). Intended as a model for other liberated areas and a shining example of United Nations benevolence, the North African relief program ran up against formidable obstacles: chronic shortages of supplies and shipping; bureaucratic confusion and infighting; the higher priority accorded military needs; and French indifference and obstructionism. The program averted economic chaos in the region, but it did little to ameliorate the plight of the Arab population or promote economic development. Despite continued strained relations with De Gaulle, the United States in 1944 worked out a novel arrangement that employed lend-lease not only to provide relief for liberated France but also to meet some of the basic needs for reconstruction of the French economy. This program was undercut by the end of the war and by congressional insistence that lend-lease must be terminated, however, and it did not begin to meet the massive needs of the devastated French economy.

Like many earlier scholars, Dougherty con-

cludes that American wartime diplomacy was characterized by a short-sighted concentration on immediate military objectives at the expense of long-range political goals. In particular, he deplores the failure of the United States to use the leverage available to it through lend-lease to promote the aspirations of Arab nationalists against French colonialism and to develop an adequate program for rehabilitation of the French economy. From hindsight, his arguments appear unchallengeable, but, at least in the former case, he seems to underestimate the practical difficulties that confronted American policy makers. The mere presence of the United States in North Africa deeply alarmed the French, and a systematic effort to encourage the Arabs or check French ambitions would have posed serious threats to the attainment of military objectives that for the moment had to take priority.

Originally a dissertation at the University of Maryland, this book could have profited from some polishing before going to press. Since the United States Army played a vital role in the North African relief operation, Dougherty might also have used Army records to advantage. And although the study is entitled *The Politics of Wartime Aid*, the connection between the various aid programs and American political objectives is sometimes unclear. Dougherty nevertheless explores an important and neglected dimension of American wartime diplomacy and adds a great deal to our knowledge of the complex relationship between the United States and France.

GEORGE C. HERRING
University of Kentucky

HANS A. SCHMITT, editor. *U.S. Occupation in Europe After World War II: Papers and Reminiscences from the April 23-24, 1976, Conference Held at the George C. Marshall Research Foundation, Lexington, Virginia*. Lawrence: Regents Press of Kansas. 1978. Pp. viii, 172. Cloth \$11.00, paper \$6.95.

Major premise: scholarly meetings should be encouraged. Minor premise: for these conferences, scholars should be encouraged to do original work. Conclusion: conference papers should be published and favorably reviewed. The logic is impeccable, as was the intent of the Marshall Research Foundation Conference in April 1976. It assembled the name scholars and their papers and then added commentary from occupation officials and commanders.

Yet the result is a mixed bag. The introduction, by editor Hans A. Schmitt, finds a continuity in the catastrophes of 1806, 1918, and 1945. Earl Ziemke provides a typically ironic conclusion, that policy went full circle, from moderation to Mor-

genthau back to moderation. The chapter, "Soviet Occupation Policy toward Germany," seems out of place and lacking, perhaps necessarily, in new sources or conclusions.

Jena Smith's high-powered analysis supports the John Gimbel thesis about Cold War origins, stressing a Robert Murphy letter of April 1946 which described the Russians as more cooperative than the French. Smith perceives a reasonable James Byrnes pushed out by a belligerent Truman and anticommunist state department. Lucius Clay's March 5, 1948 cable was wrongly used by Washington as justification for increasing military budgets. The Russians, also by mistake, stumbled into the Blockade and the Cold War.

Gimbel continues his pioneering argument on the origins of occupation policy to stress that Clay's policy was primarily a reaction to France, Britain, Congress, and the bureaucracy. Thus the Byrnes invitation of July 11, 1946 to the Foreign Ministers to join zones into one economic unit was not a reaction to V. M. Molotov's speech to entice German support but to Ernest Bevin's threat to go it alone to save British taxpayers. Gimbel also restates the thesis of his book on the Marshall Plan, that it did not spring to life in Marshall's famous Harvard speech but evolved thereafter.

Milton Colvin's article about the Austrian occupation, summarizing its major events, emphasizes the beneficial role played by Americans in containing Russians. The interesting point is that a treaty of Austrian liberation was imminent in 1948, but that American policy retreated after the Communist takeover in Prague.

The roundtable summary contains bits of criticism, particularly of Smith's argument, by the officials who remembered things differently. It is likely as good a conglomerate of opinion as could be digested into a few pages and likely justifies using officials as commentators on scholars. Less useful were the well-known remembrances of Clay and Mark Clark.

The first problem is that this tidy compilation is marred by the different levels on which it was prepared by each scholar in isolation. General materials of most are out of step with the highly specialized analyses of others. A further problem is that these once original ideas have been largely published. The delays, frequent in arranging conference publication, meant that two years passed between Gimbel's book on the Marshall Plan and this abbreviated introduction to it.

The value of conference-books is as a sampler, a scholars' digest, for those who have not the time to read full-length versions. It could whet the appetite for the real thing.

E. N. PETERSON
University of Wisconsin,
River Falls

KEN BOOTH and MOORHEAD WRIGHT, editors. *American Thinking about Peace and War: New Essays on American Thought and Attitudes*. New York: Barnes and Noble. 1978. Pp. xiii. 240. \$18.50.

These essays, by ten British and American contributors to a conference held under the auspices of the University College of Wales, Aberystwyth, retrospectively explore the intellectual consciousness of Americans in their relations with the world, principally diplomatic and military. In a sense, they are a re-examination of the ideology of the "American Century." Although their authors seek unity and pattern, they tend to find diversity and ambiguity. The value of the work as a whole lies less in any novelty of approach or data than in the fresh insights scattered throughout it.

Focusing on the convenient fiction of American Strategic Man ("nine-tenths technology and one-tenth brain"), Ken Booth tries to rehabilitate that much-maligned creature by arguing that many of the alleged failings of American strategy—for example, turning World War I into a crusade or relying today too exclusively on nuclear weapons for defense—are, in fact, in large part due to Europeans. Some American actions appear to be errors because they are judged (even by Americans) by Eurocentric criteria—as in the notion of the "belated" entry of the United States into two world wars. American alliance behavior, Harvey Starr emphasizes in a theoretical essay on that subject, is governed by a rational "unilateralism," or independence of decision.

More critical is the lucid if somewhat doctrinaire chapter by Anatol Rapoport, who sees American strategic thought as a rationalization of the needs of the military-industrial-scientific complex, a "super-organism" intent upon self-preservation. Edmund Ions discovers a key to the foreign policy of the United States in "immigrant America." As a result of this composite quality of nationhood, he asserts, interestingly but without sufficient elaboration, that "the American reaction goes well beyond the particular immigrant group in question" (p. 90).

Turning to literature (Crane, Hemingway, and Vonnegut) as a source of images of the American individual in war, Moorhead Wright finds little celebration of heroism and much evidence of the force of the "moving box" of the regiment. The civilian soldier by himself is no militant. Charles Chatfield, in a most knowledgeable and empathetic account of the pacifist strain in American thinking, questions the "dovish" label attached to pacifist groups and stresses their useful role in offering "alternative" foreign policies. He grows vague, however, when he contends that the function of peace reform is to "test" foreign policy rather than to "make" it.

James Piscatori, surveying the American legal tradition, notes a preoccupation in international-legal treatises with the concept of "neutrality" and, since the Cold War, with an "intermediate" legal status that allows for some use of force without entailing formal belligerency. The postwar state of no-war, no-peace is also recognized in the essay by Kenneth Thompson on the "ethical" dimension of American thought, illustrated in the philosophy of Niebuhr, Morgenthau, Lippmann, and Kennan. All of these, termed "pluralists" by Thompson, favor "prudence" rather than "justice" as the proper guideline of policy under modern conditions.

The two final essays deal with American "institutional" thinking: Catherine McArdle Kelleher's with its internal aspect and Inis Claude's with its external aspect. Kelleher, discussing the historic clash between the "national and Presidential" and "sectional and Congressional" perspectives, notes the "cyclical" character of the debate. Despite the 1973 War Powers Resolution, she doubts the ability of Congress to check a strong and determined president and, in fact, the efficacy of any of the "organisational" solutions to which Americans are given. Claude, paying eloquent tribute to the contributions of Americans to the cause of international organization, nonetheless questions the logic of their commitment. Paradoxically, American internationalism—contrary to the usual impression of the "American Century"—is too altruistic and utopian. Claude rightly warns that "the idealistic view of international organisation may, in the final analysis, be the source of damaging opposition to participation in it" (p. 211).

ALLAN K. HENRIKSON
*Woodrow Wilson International
Center for Scholars*

ALAN L. GROPMAN. *The Air Force Integrates, 1945-1964*. Washington, D.C.: Office of Air Force History. 1978. Pp. x, 384. Paper \$4.75.

In 1939 the U.S. Army Air Corps had no black personnel. Ten years later its successor, the U.S. Air Force, was the first service to begin full-scale desegregation, moving ahead of the tokenism of the Navy and the still recalcitrant Army. There were many factors that moved the Air Force in this direction—factors that also affected the other services. But in the Air Force there were officers in key positions who "wanted to end manpower waste" caused by segregation, and this put the Air Force in the vanguard of military integration.

Alan L. Gropman maintains that Truman's Executive Order 9981, issued in July 1948, was not the cause of Air Force integration, but merely the "catalyst" for implementation. The move toward

integration began, Gropman shows, long before Truman's political needs brought about the executive order. After World War II most military officers believed that blacks had not performed well during the war and segregation should continue. In the Air Force, however, some officers emerged from their wartime experience with different views. Colonel Noel Parrish, wartime commander of the Tuskegee air base, was convinced that segregation was not only wasteful but also unconstitutional. His postwar study of segregation influenced many in the Air Force. At this time numerous Army studies and the report of the Gillem Board (despite Gropman's claim to the contrary) favored maintaining military segregation. Racial tensions and manning problems during the war and early postwar years caused General I. H. Edwards, Air Force Deputy Chief of Staff for Personnel, to assign Colonel Jack Marr to investigate the segregation policy. Marr's report condemned segregation as wasteful and inefficient. On the basis of this study, Air Force Chief of Staff General Carl Spaatz said that segregation had to be eliminated. Assistant Secretary of the Air Force Eugene Zuckert publicly supported General Spaatz's position three months before Truman issued Executive Order 9981.

Once desegregation was achieved, race problems did not end. Gropman ably traces the story to 1971 and tells how the Air Force was slow to realize that integration had to be reinforced with positive efforts in related areas of military life such as off-base recreational facilities and education for dependents of black airmen at bases in the South. Black pressure and racial violence ultimately forced the Air Force to take additional measures to ensure against direct and indirect discrimination affecting its black servicemen.

Gropman's book raises some interesting questions to which he unfortunately does not address himself. Why, for example, did the position of key Air Force officers differ so drastically from their counterparts in the other services? But a good study will raise as many questions as it answers. Gropman's thesis that the Air Force moved ahead of the other services and the White House because of the attitudes of some forward-thinking officers is convincingly made. His book is a solid contribution to the growing literature of the period.

LEE FINKLE

*Indiana University-Purdue University
at Indianapolis*

MARK I. GELFAND. *A Nation of Cities: The Federal Government and Urban America, 1933-1965*. (Urban Life in America.) New York: Oxford University Press. 1975. Pp. xvi, 476. \$17.50.

This is a superior book, written, organized, and documented with care and clarity and illuminated by a strong and persuasive interpretive theme. It is, in fact, the best survey we have of federal-city relations and urban policy and should be required reading for public policy makers and planners as well as historians.

The items covered are simply too numerous for a brief review to summarize or even mention. Mark I. Gelfand provides a succinct overview of cities and the federal government before 1933; details the rise of a new "urban lobby" born in economic crisis and moving significantly away from the approaches of earlier municipal reformers; notes the urban impact of various public works and assistance programs; analyzes in some depth the origins and consequences of New Deal policies in the areas of urban renewal, housing, and transportation; and traces these efforts through subsequent decades and administrations down to the creation of the Department of Housing and Urban Development in 1965.

Gelfand skillfully tracks the development of legislation and programs through a maze of contending special interests and shows how some of these awkward compromises took their toll on urban America. The book makes no major revisions in our knowledge of specific legislation or the consequences of federal policy; its main contribution lies in a synthesis that is in many ways quite masterful and definitely more than the sum of its parts.

The conclusions are supported by the evidence. A revolution occurred in federal-city relations between 1933 and 1965, and on balance this revolution benefited the cities more than it harmed them. But the federal government never had an urban policy worthy of the name. Beyond this, and partly because of it, those policies that were developed were usually uncoordinated and often operated at cross-purposes. And many urban problems were compounded: suburbs expanded as inner cities declined, with the blessing of the Federal Housing Administration and other agencies; low-income housing was demolished to make way for profitable commercial schemes; highways were rammed through the urban fabric with little clear purpose in terms of overall urban design and development; and established real estate and construction interests consistently pulled federal policy in the direction of supporting the private market and, during the Eisenhower years particularly, came close to destroying public housing altogether.

Many matters of emphasis and interpretation are open to question, and a book of this scope obviously has limitations. Gelfand tends to over-emphasize the emotional tendencies of Americans toward suburbia when, given federal policies and

mass post-World War II residential construction, most homebuyers had little other choice. And one yearns for more information on the consequences of federal programs in specific local areas and situations: how these programs affected specific groups of people, boosted the growth of specific suburbs and projects, and altered delicate patterns of social interaction and lifestyle. But this is only to say how successfully Gelfand has fashioned a framework upon which others can build and expand.

BLAINE A. BROWNELL
*University of Alabama,
Birmingham*

RICHARD SAUNDERS. *The Railroad Mergers and the Coming of Conrail*. (Contributions in Economics and Economic History, number 19.) Westport, Conn.: Greenwood Press. 1978. Pp. xii, 389. \$25.00.

Every American taxpayer has a financial interest in Conrail, and the likelihood of a favorable return on the dollars invested is about the same as that for owners of Imperial Russian gold bonds. Why did the Penn Central fail, and why is Conrail going the same way? Indeed, why have the post-1945 railroad mergers generally gone sour? In this masterfully crafted volume, Richard Saunders finds that the roots of the dilemma extend back to 1904. He discovers many villains—railway management, the Interstate Commerce Commission, and Congress—and a handful of heroes. Corporate greed, bureaucratic ineptitude, and political cowardice produced bankruptcy and collapse, and the mania for mergers served only to hasten the fall.

Thoroughly researched and delightfully written, this major contribution to recent economic history describes railroad mergers in the United States from the Northern Securities Case to the first two years of Conrail. Saunders explores the vacillation and blundering of the ICC as it responded to pressure from the railroads, shippers, labor unions, and politicians. Its record is tawdry at best. Managers of many of the nation's rail systems deserve little credit as they consciously sought to merge strong lines and ignored the collapse of weak neighbors. The Pennsylvania Railroad manipulated its puppets—the Wabash, Norfolk and Western, and Lehigh Valley—to thwart rivals in the East. Every competitor of the Rock Island coveted its few profitable segments, but none wanted its debts or branches. Labor desired only job protection, and some managers gave away productivity gains to obtain union support or acquiescence for mergers, while tough management used mergers as an excuse to fire employees and discontinue benefits. Politicians wanted more tax

dollars from the carriers and improved service for their constituents, never accepting any responsibility for the financial problems created by regulation. Only a few voices could be heard urging rational mergers to develop profitable systems. The Ripley Plan of 1920 and Edward Hungerford's proposal for consolidation in 1945 were ignored, and the Transportation Acts of 1920 and 1940 offered conflicting advice to the carriers and contradictory mandates to the ICC.

Saunders contends that economies of scale have limits in railroading and shows how some profitable regional carriers became financial losers following mergers. The merger panacea often proved to be an open switch leading to financial derailment as the dockets of the ICC, which Saunders uses extensively, reveal. Rarely do historians produce books which are of such importance for the present. Written with passion and scarcely veiled anger, this study pleads for the preservation of the nation's rail system. Saunders's book should be required reading for Conrail executives, railway managers, and members of the ICC and Congress. The warnings, implicit and explicit, must be heeded or nationalization of a skeletonized rail system plagued with huge deficits seems inevitable.

KEITH L. BRYANT, JR.
Texas A & M University

JOHN H. WHITE, JR. *The American Railroad Passenger Car*. (Johns Hopkins Studies in the History of Technology.) Baltimore: Johns Hopkins University Press. 1978. Pp. xii, 699. \$45.00.

The American Railroad Passenger Car is a delight both for railway buffs and for historians of railways and technology. This extensively illustrated (black and white) and lavishly produced book by the curator of transportation at the Smithsonian Institution is a treasure trove of information of every sort on American railroad passenger cars. The book is arranged in chapters, each of which deals with a particular class or type of passenger car in generally chronological fashion. There is much technological detail but also a great deal of information on the great companies that built the passenger cars. There is a wealth of anecdotal material on the eccentricities and adventures of individual car builders, maintenance personnel, and, inevitably, wealthy patrons and owners of special cars. The focus of the book, however, remains firmly fixed on the railroad cars themselves, and the emphasis is on the important regular or working railroad cars, rather than on specially designed and outfitted cars of the wealthy and the eccentric.

Brief biographical sketches of car designers and

builders, relevant and concisely presented statistics, and a chronology of important events related to American railroad passenger cars are included in a series of helpful appendices. Extensive footnotes and an excellent bibliography provide all the references needed by even the most demanding specialist in the subject.

The American Railroad Passenger Car is a book written, designed and priced for those with a keen desire to know as much as possible about the subject. It is also an excellent reference work that specialized transportation libraries and research institutions will find indispensable. It documents an aspect of America's heritage now rapidly disappearing.

The American railroad passenger car, as John H. White, Jr., points out, never inspired a major technological breakthrough. It combined in new and specialized forms the established skills and techniques of a number of craftsmen and many manufacturing processes. Carriage makers, carpenters, wheelwrights, blacksmiths, upholsterers, drapers, painters, and steel and alloy metal workers all contributed to and participated in the building of these cars, adapting known technology to new purposes.

The most interesting of these adaptations were probably the endeavors to make the most use of available space through miniaturization and compact planning. In this respect, the railroad passenger car builders were forerunners of the automobile designers and of important aspects of space age engineering.

The decline of rail passenger service as described by White represents a melancholy chapter in American business. Conservative and lacking in innovative approaches, American railroad entrepreneurs sought to create monopolies and blamed regulatory agencies for declining rail passenger service when antitrust legislation was enforced. White describes the passenger train as "a triumph of American technology." It is not clear, however, whether its decline was due to technological obsolescence and government meddling or to unduly cautious entrepreneurship, which gave way periodically to flamboyant and expensive schemes borrowed from the airline and automotive industries.

John White's book is first and foremost an exercise in nostalgia, but it goes beyond many books of its kind in offering significant new information on, and insights into, an important aspect of American society and entrepreneurship.

T. D. REGEHR
University of Saskatchewan

PAUL BULLOCK. *Jerry Voorhis: The Idealist as Politician*. New York: Vantage Press. 1978. Pp. xi, 364. \$10.95.

Jerry Voorhis was an unusually vigorous, intelligent New Deal congressman. Yet he would seldom be mentioned if he had not been Richard M. Nixon's first victim at the polls. That defeat in 1946 conferred martyrdom upon Voorhis as Nixon rose in the world of politics. It also gave Paul Bullock, a long-time admirer of Voorhis, a topic for a biography of Voorhis as a public servant.

Voorhis's career is worth examining. If he was not of the stature of a Herbert Lehman or a Wayne Morse, as Bullock suggests, he easily ranks with such as Maury Maverick and Indiana's Charles La Follette. Voorhis was raised in Kansas, Oklahoma, Illinois, and Michigan, as his father rose in the hierarchy of the automobile industry. The major influence on him, however, was his mother's devotion to Christianity, and one of his earliest desires was "to be poor" (p. 5). Voorhis was educated at Hotchkiss School and Yale, where he was elected to Phi Beta Kappa. He tempered his Christianity at Yale with the flame of the Social Gospel. After graduating in 1923, he forsook a business career and made his living intentionally, though temporarily, as a laborer. It is not surprising, given his background and interests, that he married a social worker and soon took up a career of teaching underprivileged boys. By 1927 he became headmaster of a school in California for poor boys, which his father agreed to finance. He also found time, now as a Socialist, to champion others in distress. In 1934 Voorhis discovered Upton Sinclair's brand of Democratic politics, and he ran unsuccessfully for the state assembly. Two years later he was an ardent supporter of Franklin D. Roosevelt and, with full Democratic support, he won a seat in Congress from Los Angeles county on the issues of jobs, pensions, and Roosevelt.

During his five terms in Congress, Voorhis was diligent and articulate. He was also an idealist, which accounts for his streak of independence as was seen in his moderating role on the House Un-American Activities Committee, his opposition to deficit financing, and his reservations about Japanese relocation during World War II. Although no significant law bore his name, he did his share in helping to formulate public-interest legislation. He was, consequently, considered one of the outstanding members of Congress by 1946. Then Richard Nixon contested his reelection, and Voorhis showed his inability—as he had on several occasions in the House—to deal with a man who did not play politics by his rules. After his defeat, he served for twenty years as the executive director of the Cooperative League, where he forwarded a variety of cooperative enterprises.

Readers are indebted to Bullock for providing a good deal of information about Voorhis, who has been more important for what he represented than for what he has been. Bullock's discussions of the

problems of Christian idealists in politics, especially in dealing with political pragmatists, is worth pondering. Yet there are serious problems with his work. Most significant is that he usually takes Voorhis as Voorhis has seen himself. It is also important that Bullock, who is an economist, has used few manuscripts, large newspapers, and national magazines in his research. Although his training makes his economic discussions lucid, his long passages on recent American history and his political analyses are often biased and therefore unconvincing. He has, however, produced a work that includes the results of original interviews with Voorhis and several Nixon aides, his own recollections, and research in pertinent local newspapers. Voorhis has yet to find a biographer, but prospective biographers as well as others interested in Voorhis's career will find in Bullock some valuable data and occasionally challenging insights.

DONALD R. MCCOY
University of Kansas

DONALD F. CROSBY: *God, Church, and Flag: Senator Joseph R. McCarthy and the Catholic Church, 1950-1957*. Chapel Hill: University of North Carolina Press. 1978. Pp. xv, 307. \$16.95.

The orientation of recent historiography on post-World War II America has moved beyond McCarthyism to the larger anti-Communist impulse which emerged across a wide range of institutions and individuals. This book, focused on the narrower question of the relationship between Roman Catholics and Senator Joseph McCarthy, represents an earlier approach to domestic anti-communism. It is nonetheless valuable in demolishing once and for all the presumption of mass Catholic support for the senator, a notion current at the time and subsequently repeated by scholars.

Donald J. Crosby has made an exhaustive search of manuscript collections, religious and secular publications, public opinion polls, and secondary literature, augmented by interviews with more than forty participants in the controversy. Crosby demonstrates that McCarthy's anti-communism was rooted in political and practical, not religious, considerations, and he refutes the conventional wisdom that the senator's crusade was launched at the instigation of Father Edmund A. Walsh and his Georgetown University associates during the famous Colony dinner. Just as McCarthy avoided using his Catholicism to win Catholic votes, so did Catholics make their political choices on the basis of party rather than religion. The Catholic support for McCarthy was only slightly greater than that of Protestants, and passivity most often characterized the response of the majority of Catholics.

Such indifference, however, did not extend to Catholic leaders, among whom clergymen, editors, and educators waged a vehement debate. A major theme is Crosby's account of this conflict which saw the Catholic War Veterans and Knights of Columbus pitted against Catholic labor groups, the *Brooklyn Tablet* against *Commonweal* and the Jesuit weekly *America* (until silenced by the Father General in Rome in 1954) and Francis Cardinal Spellman against Chicago Bishop Bernard J. Sheil. Divisive and bitter though the battle, Crosby argues, the Catholic elite, as well as their divided followers, held in common a strong commitment to anticommunism and a fervent insistence that their positions represented true Americanism.

While the issue of McCarthyism fragmented leading Catholics, it also exacerbated Catholic-Protestant tensions, another major theme of this study. Liberal Protestants frequently ignored the cleavage among Catholics and portrayed McCarthy as a tool of an authoritarian church that menaced democratic institutions. Catholics, in turn, often viewed attacks on McCarthy as simple anti-Catholicism. Despite such hostilities, however, Crosby shows that on occasion liberals were able to forge ecumenical coalitions against McCarthy, and he demonstrates that rank-and-file Catholics and Protestants largely ignored the fears of their leaders.

The definitive nature of this study makes it now possible to turn from the question of the relationship between McCarthy and Catholicism to the more significant problem of Catholics and anti-communism. If, as Crosby insists, anticommunism was the major obsession of Catholics in the post-war decade, then we need an analysis of how that was manifested apart from support for or opposition to McCarthy. A deeper examination of the Association of Catholic Trade Unionists' purge of Communists and of the anti-Communist rhetoric of the liberal Catholic press, for example, might modify Crosby's characterization of such groups as civil libertarian, and it would surely contribute significantly to the most current directions in scholarly research.

SUSAN M. HARTMANN
University of Missouri,
St. Louis

ROBERT BOOTH FOWLER: *Believing Skeptics: American Political Intellectuals, 1945-1964*. (Contributions in Political Science, number 5.) Westport, Conn.: Greenwood Press. 1978. Pp. ix, 317. \$19.95.

Except for a distinct minority of advocates of natural law, legalism, and the new conservatism, American political intellectuals—important thinkers in various disciplines whose writings were rele-

vant to politics—were in one sense skeptical: they rejected ideologies and absolutes. In another sense they were unskeptical: they accepted the American liberal democratic order of “popular elections and responsibility of rulers to citizens” (p. 176). They did not think that their skepticism toward ideology in general entailed lack of skepticism toward their own ideology as they uneasily sought nonnormative middle ways between absolutism and relativism. They seldom applied the sociology of knowledge to empirical social science, and in the end they were normative because they believed postwar America was “an appropriate perspective from which to judge the worlds of human aspiration” (p. 64). They were thus parties to a paradox, “a rhetoric of skepticism and a reality of absolutism” (p. 34), identified by only a few thinkers, especially Louis Hartz.

Ethical and political relativism was attractive in the late 1940s as a “resting spot on the road from ‘ideology’ to a more acceptable rationale for liberal norms” (p. 93). Existentialism provided no absolute value justifications, but in America, if not in France, it led to acceptance of the status quo. Nonnormative logical positivism held center stage only a short time, and its middle-way variant, the system of appraisals, was doomed by its subjectivity. Analytic philosophy was unsatisfactory because of its nonnormativeness, although its preoccupation with second-order problems of linguistic clarification amounted to approval of the existing order. Its middle-way variant, the good reasons argument, “depended on liberal consensus in America to function” (p. 110).

The formulators of fallibilism and the logic of democracy shared “unskeptical assumptions about liberal democracy” (p. 116), and the method of pragmatic realism led “back to the worth of existing institutions” (p. 127). Nor did the assault on classical democratic theory create displeasure with the American political system. Individual voters were deficient, but the system of democracy was sufficient. Still, it was necessary to revise democratic theory to accord with reality while preserving democratic liberalism. Attacking the concept of majoritarianism, realists offered polyarchy and pluralism, which raised questions that the defenders of these theories dismissed so readily and unskeptically that they appeared to base their views not on empirical social science but on the seldom stated assumption that “the United States was both a satisfactory society and blessed by an unusually satisfactory political system” (p. 209).

One may ask, “Given the historical range of possibilities, what is wrong with devotion to a relatively open system that averts the horrors of ideology while allowing for improvement?” Robert Booth Fowler would respond to this question with

one of his own: “Were they [political intellectuals] skeptical enough or were they too often misled by their conviction that they were skeptical into an unskeptical conservatism at home and an unskeptical (and eventually disastrous) anti-Communism abroad?” (p. 4). This, in essence, was Hartz’s question. While overlooking skepticism in American political thought about absolute justifications of political values, about radical utopias, and about optimistic estimates of human nature, Hartz pointed to the absence, with its soporific effects, of skepticism about America or about American liberalism: “Hartz saw what the age did not see” (p. 286).

Fowler’s tribute to Hartz pleases this reviewer, whose *Consensus, Conflict, and American Historians* (1975) sees the grip of “Locke” on masses and thinkers alike and boils down to a variant of Hartz’s fragmentation theory: the Lockean ideology of the Whiggish early colonists was espoused by different, later-arriving groups, but conflict arose when the newcomers, and the freedmen, found it necessary to insist, to each other as well as to the descendants of the early settlers, that they too benefited from the prevailing ideology. Even historians who are not admirers of Hartz will appreciate political scientist Fowler’s book—especially for its treatment of historians, who, he shows, were believers rather than the hypocrites of Marian J. Morton’s *Terrors of Ideological Politics* (1972)—and his historical sense of “an era between two agendas” (p. 50), the New Deal and the Great Society. This is high praise since interdisciplinary studies usually involve one-way traffic—that is, we historians pay far more attention to the practitioners of other disciplines than they pay to us.

BERNARD STERNESHER
Bowling Green State University

BURTON I. KAUFMAN. *The Oil Cartel Case: A Documentary Study of Antitrust Activity in the Cold War Era*. (Contributions in American History, number 72.) Westport, Conn.: Greenwood Press. 1978. Pp. 217. \$15.95.

This well-researched, carefully written monograph is a useful expansion, with supporting documents, of Kaufman’s earlier article on “Mideast Multinational Oil, U.S. Foreign Policy, and Antitrust: the 1950’s” (*Journal of American History*, [March 1977]). The additional material places the cartel case in its full context, and the heretofore unpublished government documents make interesting reading by themselves.

The cartel case originated with a controversial Federal Trade Commission analysis released in 1952 under the title *The International Petroleum Cartel*.

The study, and the decade of legal proceedings which followed, charged Exxon, Mobil, Gulf, Texaco, and Standard of California with illegal restraint of trade through collusive agreements and joint ventures overseas (notably Aramco, Stanvac, and Caltex). When the case was settled in the 1960s with comparatively mild consent decrees for Exxon, Gulf, and Texaco and dismissal of the suits against Mobil and Socal, critics assumed that the reason was pressure from an inordinately powerful oil industry.

Kaufman is not an apologist for the oil companies, but his thorough study of Justice Department and related records produces convincing evidence that oil industry influence was not the determinant. From the very first, the executive branch found itself of two minds. Justice wanted full criminal prosecution, while State and Defense saw the case as undermining the very companies through which they wanted to work to secure the American position in the Middle East. As a result, Truman in 1953 directed that legal action be changed from criminal to civil, and Eisenhower's support of the Iranian consortium in 1954 forced Justice to drop its opposition to joint production and refining. Under these constraints, the best Justice could accomplish through legal action was dissolution of joint marketing in most of Stanvac and part of Caltex. Kaufman acknowledges a mutuality of corporate and government interests, but rejects the view "that foreign oil policy [in this case] was largely a response to the private interests of oil" (p. 14). Instead, the course taken was due to "the corrosive impact of the Cold War" (p. 14) and "an enlarged concept of national interest" (p. 12).

As with Kaufman's previous writings, this book is a sound, carefully reasoned addition to the growing body of literature on corporate-government relationships in the formulation and execution of foreign policy.

IRVINE H. ANDERSON
University of Cincinnati

LARRY SABATO. *Goodbye to Good-Time Charlie: The American Governor Transformed, 1950-1975*. Lexington, Mass.: Lexington Books. 1978. Pp. xvi, 283.

A general study of American governors between 1950 and 1975 is a bold venture, as the author notes with an air of uneasiness in his preface. Mindful of the hazards inherent in comparative analysis, Larry Sabato tries to make his monograph more manageable by concentrating on the similarities rather than the differences that have surfaced in the office during the last quarter of a century. This approach is intended to subserve his major thesis: namely, that the quality of gubernatorial lead-

ership has improved dramatically. In the process, he concludes that 117 of the 312 who held office during the period have been "outstanding."

Sabato supports his case by a brief historical review of changes in the constitutional status of governors, and by extensive contemporary data, including interviews with contemporary and former incumbents. The first line of argument notes the obstacles that long hampered the exercise of gubernatorial leadership and the recent developments that have increased the discretionary authority of the office. Among the latter, Sabato places the principal emphasis on constitutional revision, the more generous flow of federal money to states, and the enlargement of tax revenues at the disposal of governors. His elation over the increasing grants from Washington is unqualified by apprehensions over the constraints that have accompanied them.

The second line of argument involves a compilation of statistics on every aspect of gubernatorial stewardship that can be quantified. This exercise is intended not only to identify outstanding governors, but to sustain the prophecy that there will be no reversion to the mediocre leadership of earlier eras.

While the author sets forth suitable criteria for evaluating gubernatorial leadership, he provides no yardstick for measuring achievement. The omission is understandable in view of the enormous variables that affect the verdict in individual states. Sabato might have partially overcome this handicap by grouping states with similar political environments, establishing appropriate norms of achievement for the several categories, and reviewing in detail the career of at least one governor who met the standard of success in each category. Instead the author settles for an intriguing hypothesis and tries to document it by widely scattered references to the activities of individual governors. His impressionistic treatment will stimulate fresh interest in state executives, but a more comprehensive study is needed to validate his claim.

GEORGE H. MAYER
*University of South Florida,
Sarasota*

DAVID J. GARROW. *Protest at Selma: Martin Luther King, Jr., and the Voting Rights Act of 1965*. New Haven: Yale University Press. 1978. Pp. xiii, 346. \$15.00.

The study of Southern politics and racism by U.S. scholars continues to be a growth industry, as evidenced by this latest contribution to the literature. But the work of David J. Garrow is more than

a day-by-day account of how the historic Voting Rights Act of 1965 came into being. It is also a skillful analysis of the dynamics of protest activity and more particularly of the ways in which successful protesters deliberately use the mass media to influence uninvolved audiences.

Rightly putting description before analysis, the author begins with five background chapters that describe the Southern Christian Leadership Conference's dismal failure at Albany, Georgia, in 1961-62, the "qualified success of the eventful and often violent Birmingham campaign" (p. 3), and the Selma confrontations of early 1965 that culminated in the tragedy of "Bloody Sunday." The adoption and significance of the Voting Rights Act are then carefully appraised, after which the author proceeds to his main concern: showing how King and the SCLC ultimately brought about political and social change by deliberately manipulating the media to dramatize the numerous confrontations with the well-armed posses of Sheriff James G. Clark, Jr., of Dallas County.

Contrary to the assertion on the book's dust-jacket, Garrow's work does not really make "a major new statement on protest politics and the civil rights movement." Rather it confirms and extends the theories of such earlier scholars as E. E. Schattschneider, Harvey Seifert, and Michael Lipsky. But if the book is not particularly original, it merits high marks for its excellent integration of a wide variety of data into a unified explanation of what contributes to the success or failure of protest appeals in an open society.

Other strengths of the book are its encyclopedic mastery of the relevant scholarly literature, the massive documentation (there are ninety-one pages of notes), and the straightforward way in which the data are presented and interpreted. The principal weaknesses of the book are a rather bland style, the excessive detail in certain sections, and the failure to demonstrate conclusively that King and the SCLC deliberately and systematically manipulated the media during the Selma crisis. The author, moreover, is confronted with the inevitable methodological problems that arise when one attempts to generalize, as he does in the last chapter, from what is essentially a limited case study.

But these weaknesses are relatively minor when compared to the outstanding overall quality of the work. The strengths of the work are, in fact, so considerable that the book could well serve as a model of scholarly insight and research design implementation.

FRANCIS M. WILHOIT
Drake University

FRED HALSTEAD. *Out Now! A Participant's Account of the American Movement against the Vietnam War*. New

York: Monad Press, for the Anchor Foundation. 1978. Pp. 759. Cloth \$30.00, paper \$8.95.

Reading *Out Now!* is to relive the divisive and charged atmosphere of the sixties and early seventies. Fred Halstead, Socialist Workers Party presidential candidate in 1968, offers neither the scholar's perspective nor extensive documentation and bibliography. Rather he presents, as have others before him, a participant's history of the Vietnam era antiwar movement. The result is an effective memoir and assessment of the movement's diversity, its internal division over tactics, its numerous leaders, and its failures and successes. His extensive quoting of speeches and correspondence, compiled from printed sources or unpublished papers and interviews in the author's files (which have been donated to the State Historical Society of Wisconsin), makes the lengthy book potentially more useful to historians who will later write definitive accounts of the movement against the Vietnam War.

Halstead examines chiefly the 1965-73 period, emphasizing the diversity and slow growth of the antiwar movement among students, organized labor, the civil rights movement, and the military itself. He also points out internal leadership differences and the ultimate effectiveness of the movement. By 1973 there had been twelve years of war in Vietnam involving the United States, but the antiwar forces had produced in half that time an "overwhelming sentiment" (p. 702) against American involvement. The author is most effective in reconstructing the record-setting protest marches in the United States and abroad, giving special attention to planning, logistics (he was in charge of logistics in Washington marches of 1969 and 1971), tactics, and leadership differences. Readers are at once reminded of the absence of violence and the potential for violent clashes such as in 1968 in Chicago. Halstead traces the ups and downs of the movement coalitions during election times, military escalations, and official peace moves, stressing the debates within the movement over independent mass protest "in the streets," which he favored, rather than identifying with liberal politics and its dovish spokesmen.

Halstead clearly conveys his pride in the antiwar movement, and, writing as witness and actor, he is obviously preoccupied with his own key role. He contends that the movement was "the greatest moral resurgence in the U.S. since the struggle to abolish slavery" (p. 713) and believes its affairs were conducted "in a goldfish bowl," appropriate since it was a "genuine grass-roots" movement (pp. 715, 719). Such praise for the antiwar movement is not excessive; nor is Halstead wrong in claiming some credit for himself. His account, however, falls short in several ways. He fails to

describe adequately the finances of the movement, and the impact of peace work upon leaders' families and occupations is not sufficiently covered. Halstead seldom comments on the repressive actions taken against the antiwar movement and its leaders, stating that this "would take another volume" (p. 716). The author appropriately emphasizes the role of the Socialist Workers Party as champion of mass mobilization tactics (as opposed to confrontation tactics often desired by David Dellinger), and he correctly claims more credit for himself and other radicals than for dove Democrats and Republicans. Leadership of the antiwar forces and mobilization of thousands of Americans behind "Out Now!" was provided by radical pacifists and others influenced by A. J. Muste (mentor of Halstead and the majority of movement leaders) rather than by political leaders working in the two-party system. But the author offers a more debatable conclusion in urging the possibility that "the antiwar movement will prove to have been in a number of aspects a rehearsal for the coming American socialist revolution" (p. 729).

ERNEST C. BOLT, JR.
University of Richmond

CANADA

ANDRÉ LACHANCE. *La justice criminelle du roi au Canada au XVIII^e siècle: Tribunaux et officiers*. (Les Cahiers d'Histoire de l'Université Laval, number 22.) Quebec: Les Presses de l'Université Laval; distributed by International Scholarly Book Services, Forest Grove, Oreg. 1978. Pp. xvi, 187. \$10.75.

André Lachance's last book will be very useful to all those interested in the study of New France. It deals with an aspect of the past neglected until very recently by serious researchers. Criminal justice is a new, and very promising, field of research for sociocultural historians.

The author has limited his investigation to the years 1712 to 1748, roughly the last peaceful period of the French régime. In the first part of the book he describes the judiciary system at the beginning of the eighteenth century. There were four royal courts in the colony: the Prévôté of Québec, the Royal Courts of Trois-Rivières and Montreal, and the Superior Council of Québec, which functioned as a court of appeal. Following an examination of the role of each of the members of the magistracy, Lachance reviews the social composition of the group. Royal prisons are then studied in a brief chapter. The second part entitled "The Practice of Justice" contains two chapters: the first offers a detailed account of the inquiry and the trial; the

second describes the diverse punishments in use at the time.

The research has been seriously conducted. French legislation has been carefully studied, the judiciary archives of New France well analysed. The writing is clear and usually good. All aspects of justice are covered in the analysis and, when possible, quantified. The author's good judgment and his taste for concrete details is revealed in his choice of facts. The tables and graphs he has prepared illustrate well the points he makes. Table two, for example, gives the socioprofessional origins of the fathers of these men, and table three shows the matrimonial alliances of the latter.

On the negative side the author does not clearly explain in his introduction the purpose of his work. On page four he says: "Our goal is to grasp how in Canada justice was rendered by lay judges, and who these men of justice were." He proposes a study that is at once institutional and social. His approach to the institutional dimension of the judicial system is both theoretical and practical. However his methodology is not clearly defined. He makes insufficient use of concrete examples and quotations. Our frustration is compounded by the dramatic quality of the few texts that are quoted—like the one reporting the words of the male prisoner being tortured with the "brodequin" (laced boots). The author draws no conclusion from this striking dialogue between a ruffian and his interrogator (p. 82). Insofar as the social study is concerned, one would like to know what exactly the author means by social analysis, and why instead of a detailed inquiry he gives only a short outline.

His comparisons to the judicial system in England and New England are deceptive. In order to convince us that the penal system was "much less strict in Canada than in Great Britain and in New England," the author should give more substantive evidence (pp. 130-31). Moreover, the comparison with the practice of the English colonies should have been pushed much further.

As remarks of a minor nature, I observe that because the percentage of those who could sign their name among the delinquents is not compared with the main body of the population (p. 136, n. 4) it is not possible to judge the significance of the statistic. I regret the absence in the bibliography of Yves Castan's *Honnêteté et relations sociales en Languedoc* (1974). But all in all the book is very interesting, and its reading will benefit all those who study colonial life in North America.

JEAN-CLAUDE DUBÉ
University of Ottawa

CRAWFORD KILIAN. *Go Do Some Great Thing: The Black Pioneers of British Columbia*. Vancouver: Douglas and McIntyre. 1978. Pp. 188. \$12.95.

This book is unscholarly, undocumented, naive, and out-of-date, in part because the writer's background consists of authoring children's books, science fiction stories, and technical reports for the Lawrence Radiation Laboratory. It is out-of-date because of James Pilton's unpublished Master's thesis, "Negro Settlement in B.C., 1858-71" (1951), which Crawford Kilian read with great care. The swift result was *Go Do Some Great Thing*, which Kilian did not do.

His introduction is a naive 1960s diary of his discovery of British Columbian bigotry (p. 11). This happened to Kilian "by chance," and that is how he became interested in the subject (p. 11). Yet, having become interested, he found that "some writers had distorted the facts" (p. 11), though he never names them. Obviously upset with historical bias against blacks (p. 12), Kilian capitalized black throughout his book. Ending his introduction, he implored a "professional historian" to re-examine the subject "and correct the errors of fact and interpretation" (p. 13).

His book actually contains few errors of fact or interpretation because he studied Pilton well. Concerning the much discussed Archy Lee case, Kilian follows Pilton closely (Kilian, p. 12; Pilton, p. 21). Concerning the Blessing murder, Pilton writes (p. 167) "who was impatient to be on his way," while Kilian writes (p. 91) "who was impatient to go on." His bibliography shows he uses some new sources, but he and Pilton tell the same story.

Not surprisingly, Kilian's thesis (p. 158) is the same as Pilton's (p. 1), namely, that blacks were tough and resourceful in facing Canadian racism. This thesis is argued convincingly in chapters on racial discrimination in churches, politics, society, schools, theaters, and between Indians and blacks. Further, he demonstrates that gold rush regions, primarily Cariboo, treated blacks the same as whites (p. 95), but he never accounts for this irregularity and ignores most race-related frontier historiography (for example, Rudolph Lapp, W. Sherman Savage, and Eugene Berwanger). The chronology starts in 1858, when 600 blacks left San Francisco for the Vancouver gold fields, and ends with the Civil War, when racism intensified and half of the blacks left for parts unknown. A 1907 race riot is tacked on for good measure as is a discussion of recent racism in Canada. The book has this virtue: it is actually about some blacks, which much of black history is not.

Other than that, perhaps it should not have been published; it was already written, and scholars in the area know Pilton. Meaningless diversions abound on Indian prostitution and geography; so do silly statements, such as the one claiming that the black exodus from San Francisco to British Columbia "would foreshadow that of . . . the war

resisters and deserters of the 1960s" (p. 16). Kilian concludes that blacks were "an island of stability" (p. 49), "epic figures, enduring hardships and giving immense, patient love" (p. 101). Pilton is better.

GERALD STANLEY
California State College,
Bakersfield

DAVID J. BERCUSON. *Fools and Wise Men: The Rise and Fall of the One Big Union*. Toronto: McGraw-Hill Ryerson. 1978. Pp. xvii, 300. \$6.95.

University of Calgary historian David Bercuson offers us here his most recent exploration into Canadian working class radicalism. His history of the One Big Union extends his earlier work on Winnipeg workers and on their 1919 general strike. It also constitutes an interesting complement to another recently published work on western Canadian radicalism, Ross McCormack's *Reformers, Rebels and Revolutionaries* (1977).

Bercuson's book opens with background chapters on the composition of the western Canadian working class (especially the resource proletarians of the lumber, coal, and metal mining industries), on the west's "unique fermentation" of immigrant groups, on the domestic social effects of World War I, on the nature and growth of the Canadian socialist movement, and finally on the worldwide syndicalist upsurge of this period. These chapters are strongest when most specific about the conditions faced by western workers; they are weakest when trying to place this western regional experience in its national, continental, and international context. The relatively weak discussion of Australian, American, and English syndicalist traditions appears to be based on a rather selective reading of secondary sources. This important limitation takes on great significance since the Canadian west's "unique fermentation" appears rather less singular when placed in the context of the world-wide workers' revolt.

The book then chronicles the formation of the OBU, its participation in the general strike wave of early summer 1919, and its civil war with the American Federation of Labor and Trades and Labor Congress of Canada (TLC) for the hearts and dues of loggers, coal and metal miners, and urban workers. Bercuson describes extremely well the overt cooperation of Canadian and American capitalists, the Canadian state, and the TLC and AFL in smashing the OBU.

In an intriguing, reverse organizing thrust, the OBU invaded the U.S. and enjoyed some success not only in the far west but also among Chicago railroad shop workers and among Lawrence textile

workers. These American successes and similar efforts in central Canada raise additional questions about the "unique fermentation" thesis and especially about the role of urban skilled workers in the syndicalist upsurge. Surely there is considerable significance in the role machinists played both as OBU leaders and as the skilled group most receptive to OBU ideology. A close examination of the machinists' work-world might have led to a rather different interpretation of the OBU experience. Here David Montgomery's work on "the new unionism" in America raises important questions not considered by Bercuson. The western exceptionalism argument also encounters difficulties with the OBU's mid-1920s organizing drive in the Nova Scotia coal fields. Here OBU western organizers met coal miners whose radicalism, although not a product of any western fermentation, ran equally deep.

In accounting for the OBU's ultimate failure, the author's zealous dislike of the Socialist Party of Canada provides him at times with a scapegoat and thus a too simple analysis of the movement's demise. Moreover, his general impatience with the left-wing debates of the period leads him sometimes to dismiss them without adequately assessing their significance. Nevertheless, he does capture well the irony in the OBU's decline until it represented only a rump of Winnipeg workers loyal to the heritage of the general strike and to the personal leadership of Bob Russell.

Bercuson's book, the first monograph on the OBU will stimulate new debate on the nature of the postwar Canadian workers' revolt. It should also interest labor historians in the U.S., England, and Australia for the comparative perspective it will suggest to them. A well-written, professional account of some elements of the OBU experience, the book nevertheless reminded this reviewer of William Preston's rhetorical question of Melvyn Dubofsky's *We Shall Be All: A History of the Industrial Workers of the World* (1969), namely "shall this be all?" Bercuson details the major incidents of the workers' revolt but fails to evoke their passion and potentiality. W. A. Pritchard's fools and wise men deserve better.

GREGORY S. KEALEY
Dalhousie University

PAUL RUTHERFORD. *The Making of the Canadian Media*. (McGraw-Hill Ryerson Series in Canadian Sociology.) Toronto: McGraw-Hill Ryerson. 1978. Pp. xi, 141. Paper \$5.95.

While there are many books and articles dealing with various aspects of the history of communications, very few of these can be considered signifi-

cant contributions to our understanding of the actual function of communication systems in the process of historical change in societies. In recent years there has been a mild revival of interest in the history of mass communication, but we are still woefully lacking in major works in this field. Paul Rutherford, a social historian at the University of Toronto, has made a modest, but interesting, contribution to this field with this slim volume on the growth of the communications media in Canada.

Rutherford is careful to note that he is not to be considered a "communications determinist." He clearly states that he "does not mean to suggest . . . that the media have been any more central to the course of Canadian development than the business community, political parties, the churches, the family or, for that matter, the men's clubs." What he does provide, nevertheless, is an excellent, somewhat opinionated survey of the historical development of the mass media as an integral aspect of the evolution of Canadian society.

A book of this nature runs several risks. It could be too ambitious and try to interweave the history of the various communications media, with the result that the overall assessment becomes convoluted and inaccessible. On the other hand, it could be overly simplistic, concentrating merely on the basic narrative history, while ignoring the social and cultural impulses that underlay the growth of the various mass media. Happily Rutherford's book walks the line between these two extremes; it is a short, clearly-written, accessible work that offers the non-specialist a guide to the history of the Canadian mass media in a socio-cultural context.

The book is divided into three large chapters or sections; "The Rise of the Newspaper," "The Golden Age of the Press," and "The Triumph of the Multimedia." Each section pulls together diverse narrative threads, but Rutherford's background as a social historian is evident here, and he is thus able to relate media developments to the relevant historical facts. This allows the reader to understand how the various media of communication developed in response to social and cultural tensions.

One positive aspect of the book is that Rutherford has taken the trouble to read the sociology of mass communications, and therefore he avoids the wide-eyed claims for "media impact" made by so many historians. He has also taken the time to examine in their original form most of the historical press he discusses, and this makes his account much more lively and pertinent.

Finally, much of the strength of this short volume lies in the author's method of examining the various media in their correct socio-cultural context. He is not highly critical of the media and

their content, but instead he views each in the light of their public function. All in all, this is a very useful introduction to the history of the Canadian mass media and could serve a variety of useful purposes for historians and media specialists.

GARTH JOWETT
University of Windsor

LATIN AMERICA

CHRISTIAN DUVERGER. *L'esprit du jeu chez les Aztèques*. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques. Civilisations et Sociétés, number 59.) Paris: Mouton. 1978. Pp. 298. 130 fr.

The title of this intriguing and provocative but sometimes exasperating book does not adequately convey its large scope and subject matter. It is not primarily a study of the forms that play (the word *jeu*, combining the meanings of "play" and "game," poses a problem of translation) assumed in Aztec society. Its more ambitious aim is to reveal the Aztec mentality as reflected in the Aztec attitude toward play and the role of play in Aztec life. Written under the direction of Jacques Soustelle, this thesis shows the influence of his well-known ideas on the character and evolution of Aztec society.

Christian Duverger's thesis may be briefly summarized. Play could have had little place in Aztec life during the long march over the northern plains in search of a promised land or during the difficult struggle for survival after the Aztecs arrived in the Valley of Mexico. In time, Aztec military and political success, and the growth of population and division of labor, released the resources and leisure time needed for recreational activity, especially on the part of the nobility. The tribal leadership, however, regarded play with suspicion as a subversive activity that wasted vital forces and thereby contributed to the process of "natural entropy" that must some day end in the destruction of the universe. But the astute Aztec leaders, experts in social control, did not ban play outright; they were content to restrict its influence by denouncing its most antisocial forms, such as gambling, and by integrating a wide range of recreational activities with the ritual of human sacrifice, regarded as a liberation of energies that could reverse or at least stabilize the process of entropy.

By the opening of the sixteenth century, the cessation of large-scale war and the growing wealth and luxury of the ruling classes had made Aztec life less austere, more "civilized," and this contributed to an upsurge of secular play. Side by side with the sacred ball game (*tlachtli*), which always ended with the sacrifice of the captain of

the losing team, there now existed a secular ball game of the same name; another entertainment, derived from a rite of divination, was the game of *patolli* (resembling parchesi or backgammon). Duverger claims, however, that these Aztec games lacked an essential element of their Western counterparts: competition could not exist in a society governed by a sacred almanac that determined men's fates; consequently there could not exist teams or clubs that competed with each other. The same determinism excluded true betting; and Duverger suggests that betting must have been primarily an activity of the nobles because, knowing that they had been born on lucky days, they could gamble without fear of ruin.

But the *Historia de las Indias de Nueva España* of Fray Diego Durán, to whom we owe the most detailed description of Aztec games, obtained from surviving Aztec elders, offers a very different picture. Durán asserts that all major cities and towns had ball courts and teams. Outstanding players were honored with special dignities and insignia. Durán does not support Duverger's thesis that gambling was a peculiarly aristocratic practice and had a semiclandestine character when indulged in by commoners. Durán's description of *patolli*, "their most common game," has a distinctly plebeian flavor: "When this game was played, such a crowd of onlookers and gamblers came that they were pressed against each other around the mat, some waiting to play and others to bet." Durán adds that players, when casting their dice, called on Macuilxochitl, the god of the game, to bring them luck—an act that does not suggest an absolute belief in predestination.

Duverger's discussion of these issues illustrates his general tendency to force facts into a rigid theoretical frame and to make highly speculative assertions. Is it not a Jungian extravagance to suggest that the Aztec taste for hallucinogens was an "ancestral atavism" brought from their supposed place of origin in eastern Siberia near the Bering Strait? Is it not misleading to ascribe to the Aztecs a belief in "natural entropy" (that is, a running down of the universe) when the Aztec myth of the four or five suns or ages clearly assumes a sudden catastrophic destruction of the world? Such dubious statements weaken a book that otherwise is valuable for its large factual content and important theoretical insights.

BENJAMIN KEEN
Northern Illinois University

TIMOTHY E. ANNA. *The Fall of the Royal Government in Mexico City*. Lincoln: University of Nebraska Press. 1978. Pp. xix, 289. \$15.00.

Revisionist writing that is cogent and carefully reasoned, neither polemical in tone nor ideological

in motivation, should be given respectful attention and thought. Timothy E. Anna's book falls into this category and therefore merits careful consideration. In analyzing the Mexican movement for independence, Anna offers a significant modification of the heretofore generally accepted counter-revolution theory that falls just short of rejecting it altogether. And he does so persuasively. Suggesting that the final achievement of independence was neither revolution nor counterrevolution, he argues that it was "a massive compromise of such proportions that it satisfied no one beyond the immediate moment" (p. xiii). This compromise briefly reconciled "the objectives of the long-time rebels with the more limited aims of the elite" (p. xiii).

Anna argues that until now the historical focus has been on the rebels and the rural areas and that the neglect of Mexico City has constituted a serious omission. The nature of society in the viceregal capital was critical to the course of the independence movement and therefore it must be analyzed with reference to the sequence of events from 1808 to 1821.

Anna builds his case slowly and carefully, marshaling both socioeconomic and political arguments. He analyzes the society of Mexico City on the eve of the outbreak in terms of class, based essentially on economic status. He discerns four classes—royal administrative and foreign elite, domestic elite or plutocrats, petty bourgeoisie, and poor—and effectively describes their life-styles with reference to how these determined shifts in political allegiance throughout the long struggle.

He also explores the distinction between authority and power, which he regards as critical to a valid explanation of the shift in sentiment among the power elite in Mexico City whose support was essential to the achievement of independence. A corollary of this factor was the concept of autonomy rather than total separation from Spain.

In tracing the course of the movement, Anna finds Iturrigaray to have been not so much villainous as confused. He argues that Venegas and Calleja, in resorting to force that finally became unwarranted, maintained royal power but destroyed royal authority. Apodaca sought conciliation and the restoration of that authority, but his intentions were frustrated by the ill-considered actions of Ferdinand VII.

Although Anna builds on other revisionist works, especially those of Doris Ladd and David Brading, his own contribution is notable. The subtlety and abstract nature of his analysis make it somewhat difficult to encapsulate, but his provocative and significant study should be carefully pondered by all interested scholars.

BERNARD E. BOBB
Washington State University

MICHAEL P. COSTELOE. *Church and State in Independent Mexico: A Study of the Patronage Debate, 1821-1857*. (Studies in History.) London: Royal Historical Society; distributed by Swift Printers, London. 1978. Pp. x, 207. \$17.00.

Michael P. Costeloe, highly regarded for his studies of nineteenth-century Mexico, adds to his reputation with this solidly researched, detailed history of ecclesiastical patronage in Mexico from Independence to the reform. Costeloe's objectives, which he has achieved, are to present an accurate account of the course of the patronage controversy from 1821 to 1857; to present an evenly balanced examination of the ideas of the clergy and the reformers; to identify the patronal powers and suggest why patronage aroused so much conflict; and to place the struggle between Church and state within the context of Mexico's changing society.

The outlines of the patronage issue, the conflict and its consequences, are familiar enough, but this is the first occasion that the important question has been the central theme of an objective and nonpartisan study. Costeloe carefully explains the origin and evolution of the regalist and canonist views of patronage, differences among adherents of each position, and the dilemmas facing both groups that led to inconsistencies and contradictions in their positions. The author believes that the question of ecclesiastical patronage was at the heart of the broader struggle between liberal reformers and conservative supporters of the Church and that an understanding of the patronage issue is essential to an understanding of the actions of the clergy and the reformers. Costeloe does, however, keep the issue in focus, within the context of other matters of importance in Mexican affairs.

The author explains developments regarding patronage on the national level, and he relates them to controversies in the states where some of the most strident voices of reform were heard. He discusses papal attitudes toward independence and patronage, as well as detailing the sporadic negotiations between the papacy and successive Mexican governments toward a concordat, which was nearly achieved in 1838. By that time, however, in order to meet the needs of the faithful, informal agreement had at least been reached with regard to appointments to vacant benefices.

The entire complex issue was finally resolved in the liberal 1857 constitution, which embodied the extreme regalist position on patronage. Ultimately, however, in 1859, separation of Church and state was proclaimed, thus settling the long-standing controversy in a way few would have considered likely or desirable prior to the reform.

Costeloe treats this subject with his customary thoroughness, precision, and clarity, but the com-

plexity of the topic makes for some difficult reading. Costeloe pays scant attention to the patronage question after 1851, and he ignores the imperial interlude in the 1860s when Maximilian claimed all patronage rights formerly exercised by the Spanish monarchs. Nonetheless, this is an excellent study that merits a place alongside other significant monographs on various aspects of Mexico's early post-Independence history.

ROBERT J. KNOWLTON
University of Wisconsin,
Stevens Point

OSCAR J. MARTÍNEZ. *Border Boom Town: Ciudad Juárez since 1848*. Austin: University of Texas Press. 1978. Pp. xvi, 231. \$12.95.

A major criticism of *Border Boom Town: Ciudad Juárez since 1848* is that the title is misleading; the work does not give a clear history of Juárez itself. The text, in fact, jumps continuously from Juárez to El Paso and to other frontier areas in a manner that is irritating. In the process, *Border Boom Town* omits major themes and events in the development of the Juárez-El Paso area such as the "Salt War" of the 1870s, the incessant border conflicts of the nineteenth century, and the development of ruling elites in the region. The impact of events in the Mexican interior on the development of Juárez is also ignored.

A strong point of the work is its original research on topics generally ignored by traditional historians. Oscar Martínez discusses, for example, the struggle of Juárez to achieve economic independence from the United States through programs such as the *Zona Libre* (Free Zone) along the border during the nineteenth century. Through this policy federal authorities exempted border merchants from high import duties, giving them an economic advantage over their U.S. counterparts. The program infuriated U.S. merchants who urged their federal representatives to pressure the Mexican government to end the *Zona Libre*. The demise of the Free Zone in the early 1900s set a pattern for U.S.-Mexican relations that amounted to economic vassalage of *la frontera* to the United States, with border towns such as Juárez reduced to "catering to U.S. lawlessness"—evasion of Prohibition, gambling, cheap liquor, and prostitution.

Border Boom Town contributes to the growing body of research on the repatriation of an estimated half million Mexicanos during the Great Depression. Martínez shows the reader how the program affected Juárez-El Paso, which was the most popular way-station for returning Mexicans, and he does a good job of describing the sufferings of the destitute families involved.

A thorough knowledge of the history of Mexican migration is essential to advancing an intelligent policy. The simplistic solutions advanced by the U.S. press and politicians are exposed by Martínez's narrative of the *Bracero*, the Green Card Commuter, and Border Industrialization programs. The latter programs magnetized the *frontera*, swelling the populations of the border municipalities and making them more dependent on the United States.

The author describes the current economic dependence of Juárez on El Paso as well as the struggle of Juárez merchants to reverse the cash flow out of their city. Martínez shows how the merchants succeeded in stopping the quaint low-cost trolley that connected the two cities. Interestingly, while the street car appeared to take tourists into Juárez, in reality, it transported Mexican shoppers into El Paso, draining Juárez of needed capital. Further, the Mexican government resurrected a modified version of the Free Zone in an attempt to reverse the trade flow. It cut the duties on popular U.S. products, *Artículos de Gancho* (articles of enticement) in order that Juárez merchants could compete. The most effective measure, however, has been the devaluation of the Mexican *peso*, which has reversed the cash flow between the towns but has also made Mexican labor even cheaper for industrialists.

Border Boom Town is a readable, scholarly work which is recommended to scholars of the region. In spite of major flaws—the work is too broad and too thin in parts—it does treat important issues and events.

RODOLFO ACUÑA
California State University,
Northridge

JOHN M. HART. *Anarchism and the Mexican Working Class, 1860-1931*. Austin: University of Texas Press. 1978. Pp. x, 249. \$14.95.

If one can believe John M. Hart, anarchism, or, as he says, the libertarian socialist movement, left a marked imprint on the history of the Mexican working classes from the middle of the nineteenth century through the 1920s. Not only does he argue for its influence among the emerging urban working class, but also for its impact on the agrarian movements of the era and later even on the Zapatistas in Morelos. He traces its roots to Mexico's own "unique developmental process" (p. 3) and to European influences, among them the writings of Pierre Joseph Proudhon, Michael Bakunin, and Peter Kropotkin. But, as Hart defines it, anarchist thought becomes an umbrella big enough to shelter nearly every unorthodox scheme, no matter how poorly understood by Mexican workers. Hart

clearly is a historian who believes that ideas move men, even illiterate and semiliterate workers and dirt farmers. Although recognizing conditions unique to Mexico that account for its rise, he never satisfactorily explains why anarchism, regardless of how he defines it, flourished (if indeed it did) in soil so distinctly different from its home base in Europe. This, in my opinion, is one of the principal weaknesses of this small book.

Hart's study is, in fact, mistitled for he has not written about anarchism and the Mexican working class but about a small group of budding ideologues, some of whom became nabobs while contributing to Mexican efforts to build an independent labor bloc. The thoughts of factory workers and dirt farmers, or peasants, as they were called, is left to the realm of speculation. Despite his title, Hart never adequately demonstrates that anarchist theory led workers and peasants to confront Mexico's rulers and employers. At best anarchism was the gospel of a small minority of labor spokesmen. Gut issues—demands for better pay, shorter hours, and decent working conditions—probably better explain why workers and peasants rebelled against their masters.

The strength of Hart's study lies in the section dealing with the era from 1861 to about 1916. Here, in careful detail, he charts the course of the labor leaders influenced by exotic ideas imported from Europe, usually by way of Spanish ideologues who arrived in Mexico at the turn of the century. The pinnacle of his story comes with the founding of the *Casa del Obrero Mundial* in 1912 and its subsequent efforts to unite workingmen into a national labor movement. History, Hart acknowledges, records the failure of the *Casa's* efforts, which he attributes largely to the hostility of the Constitutionalists, Venustiano Carranza, and his allies. Yet Alvaro Obregón, labeled prolabor and a Constitutionalist by Hart, watched the demise of the *Casa* and of the nascent labor movement without uttering a protest, a point that Hart ignores. Later, both Obregón and his successor, Plutarco Elías Calles, showered favors on the CROM of Luis Morones while turning their backs on the CGT, the refuge of many of the old anarchists.

Hart's monograph, essentially a political history, nevertheless calls attention to the importance of ideas and theories in the early Mexican labor movement. As he implies, Mexican labor thought did not develop in a vacuum but in conjunction with the currents of the age. Anarchism, and its companion in thought, socialist doctrines, failed because they were out of step with Mexican economic and political reality.

RAMÓN EDUARDO RUIZ
*University of California,
San Diego*

JESSIE PETERSON and THELMA COX KNOLES, editors.
Pancho Villa: Intimate Recollections by People Who Knew Him. New York: Hastings House. 1977. Pp. xvi, 279. \$12.95.

There are two very different aspects to the personality and activities of Francisco "Pancho" Villa. The first includes his life as a bandit and cattle rustler before 1910 and his activities as a guerrilla fighter after his defeat in 1915 (especially his attack on Columbus, New Mexico, in March 1916). The second concerns his role as a major political and military figure in Mexico between 1913 and 1915, when he commanded the largest and best equipped army in the country, controlled large parts of the Mexican north, and, for a brief time, held sway over most of the country.

The first aspect has been the subject of far more books and articles than the second, and we still do not know what forces Villa really represented and what social changes (such as agrarian reform or measures regarding labor) he did or did not implement while in power. This book does not deal with the second aspect of Villa's activities at all and does so only to a limited degree in respect to the first. With very few exceptions, such as Villa's wife, Soledad Seañez, the thirty-one "intimate" recollections are by Americans or Mexican-Americans. There is, on the whole, far more hearsay than actual recollection in the book. Some witnesses, such as Cleofas Calleros, scarcely knew Villa, while others, such as Dr. R. H. Ellis, who knew him well, barely mention personal recollections.

The statements of these witnesses confirm some well-known traits of Villa's personality: he could be extremely generous or cruel; he was kind to children but had few compunctions about killing prisoners or those he thought had betrayed him; he was uneducated but respected education greatly. No attempt has been made by the editors, Jessie Peterson and Thelma Cox Knoles, to find out what social ideas he held or what solutions, if any, he advocated for Mexico's problems.

In view of the time that has elapsed since the events that were viewed by the witnesses, errors and lapses of memory could easily occur. The editors could have corrected these by follow-up questions. Thus, for instance, a witness, Carmen Visconti Abel, remembers Villa's troops committing depredations in Chihuahua when she and her family were being evacuated from that state in December 1912. At that time, though, Villa was escaping from prison in Mexico City to El Paso and had no troops under his command. The witness either mistook the date or attributed depredations committed by other troops to Villa's men. An error such as this (and there are many) could

easily have been eliminated by follow-up questions by Peterson and Knoles.

The book is of very limited value as a source for understanding Pancho Villa and what he stood for. But in a quite different way and on another level from what the jacket of the book and the introduction claim, it does constitute a valuable source. It very clearly shows the attitudes of Americans living in northern Mexico and in the border area toward the Mexican Revolution and toward Pancho Villa in particular.

For many Americans, such as Susan Knotts, whose husband Villa held for ransom, the Mexican leader was "a brutal, vicious monster." For a great many other Americans, he was one of the great Mexican revolutionaries. They see him as the one man who provided them with protection and who honestly wanted to improve the lot of Mexico's poor. They condemned Woodrow Wilson's decision to recognize Venustiano Carranza as the head of the Mexican government and, above all, the permission he granted Carranza's troops to cross U.S. territory in order to defeat Villa's forces at Agua Prieta. A surprising number of witnesses refused to believe that Villa participated, or was even involved, in the attack on Columbus, New Mexico. The book thus helps to elucidate the complex relationship that developed between Americans living in the border area and Mexican revolutionaries.

FRIEDRICH KATZ
University of Chicago

HEATHER FOWLER SALAMINI. *Agrarian Radicalism in Veracruz, 1920-38*. Lincoln: University of Nebraska Press. 1978. Pp. xviii, 239. \$15.00.

Heather Fowler Salamini's book represents both a victory for the regional historiography of twentieth-century Mexico and a disappointing failure for an overall understanding of Mexican politics during the 1920s and 1930s. Her work concentrates on the rise and fall of the Veracruz Peasant League during the heyday of the political chieftain Governor Adalberto Tejeda. Organized by two members of the Mexican Communist Party, the League later came under the protective wing of Tejeda, who used it to build a fully-armed power base from which he launched a determined but fruitless drive for the Mexican presidency in 1933-34. Tejeda's problem was that his extremism on such issues as land reform, religious persecution, and taxation of foreign oil companies antagonized and frightened the so-called revolutionary leaders in charge of the national government, namely Plutarco Elías Calles and, after 1934, President Lázaro Cárdenas. Calles and Cárdenas always remained at odds with Te-

jeda, and Cárdenas used certain powers at his disposal to form the National Confederation of Peasants, which virtually absorbed the Veracruz League. The League no longer enjoyed the protection of Tejeda, who had been politically isolated and then sent abroad as an ambassador—mainly it seems to get him out of the country and thus out of the ruling party's way.

Part of the Veracruz peasantry did benefit substantially from Tejeda's programs, but Salamini strays from reality when she persistently extols Tejeda as a model of true "democracy" while casting aspersions (via pejorative language) on Mexico's national leadership. Tejeda's conduct in office and his political philosophy did not even remotely advance toward democracy, as that term is understood in the Western world. His two governorships (1920-24; 1928-32) were characterized by atrocities against Catholics and their clergy that remind one of Emperor Nero rather than any of the numerous true democrats then active in Mexican politics (mainly old liberals alienated by the statism and demagoguery of the official party). Tejeda, moreover, stood as close ideologically to Stalinist Communism as any Mexican could without actually joining the Mexican Communist Party. The former governor maintained his predilection for Communism while serving as ambassador to France and later to Republican Spain; his unpublished 1939 report on the Spanish Civil War amounts to an almost verbatim repetition of the Stalinist "line" on Spain.

While the author correctly states that Tejeda's radical agrarianism would have brought about a genuine social revolution in Mexico (as opposed to the pseudorevolution of Calles and Cárdenas), her claim that his program, enacted in Veracruz and offered to the nation as a whole during Tejeda's quixotic run for the presidency, would have resulted in a new, "humanistic" Mexican democracy is pathetically naive. Like Calles, Tejeda's face had "boss" stamped all over it. If Salamini had been able to jettison her obvious sympathy for the man and his policies, she might have found the real Tejeda and have written a more enlightening book.

T. G. POWELL
State University College,
Buffalo

SILVIO ZAVALA. *Orígenes de la colonización en el Río de la Plata*. Mexico City: Editorial de El Colegio Nacional. 1977. Pp. ix, 708.

It is unfortunate that this book appeared in a limited edition of only 1,000 copies. The contents of the volume and Silvio Zavala's deserved reputation

as one of the deans of Latin American history merit a much wider audience. The author uses the word *origenes* broadly, encompassing not only the entire sixteenth century but a quarter of the seventeenth century as well. This is as it should be, for most assuredly the "Conquest" of the Río de la Plata was an ongoing affair of many generations of effort.

The text is divided into three basic sections: "The Expeditions of Conquest," "Treatment of the Indians by the Settlers," and "European Colonization." Although one could perhaps wish for more depth concerning the regional Indian cultures, the author argues the idea that "a community with historical roots is something more than a hodgepodge of settlers or a web of economic activities . . . it is also a spirit . . . How did the first settlers of Asunción experience the life of that town? What image did they have of their function and destiny?" (p. 523).

For Zavala, these are not rhetorical questions, and he does an incisive job of introducing the human element throughout this work. Also to be lauded is his proper interpretation (both geographical and historical) of the region of the Río de la Plata. He avoids the common trap of focusing almost exclusively on Buenos Aires and the Argentinian northwest, and instead deals at length with every segment of the subcontinent. Most pleasing in this context is the author's treatment of Paraguay, so often ignored in volumes of this nature. Paraguay was settled earlier, had a greater population base, and a larger proportion of sedentary Indian labor (Guaraní) than any other part of the Río de la Plata.

The primary research for this volume was carried out more than three decades ago, principally in the *Archivo General de la Nación* (Buenos Aires) and the *Archivo Nacional* (Asunción), but the bibliography and notes indicate that the author has consulted virtually every collection of printed primary sources relevant to the subject. One could cite a weakness, however, in the author's slighting of monographs and articles published in the last five years; some of these could have been useful.

The utility of this volume is greatly enhanced by exceedingly detailed indexes of people (various spellings of names laboriously cross-indexed), places, and things. The seventy-two pages that the author allotted for his indexes make this work a true *obra de consulta*.

JOHN HOYT WILLIAMS
Indiana State University

ROBERT M. LEVINE. *Pernambuco in the Brazilian Federation, 1889-1937*. Stanford: Stanford University Press. 1978. Pp. xxii, 236. \$17.50.

Several words about the overall project to which this book belongs are indispensable. In the late 1960s Joseph L. Love, Robert M. Levine, and John D. Wirth began work on parallel regional studies of Brazil between the late empire and World War II. They chose states exemplary of subnational units in developing countries—in this case the dynamic São Paulo (coffee and industry), the decadent Pernambuco (sugar and cotton), and the heartland Minas Gerais (mining and agropastoral activities). In the period chosen states gained far more autonomy than at any other time, but they also witnessed wide fluctuations in their fortunes and gradual integration under a stronger central state after World War I. The researchers used common definitions of regionalist phenomena and sophisticated tools to facilitate comparison.

Levine's volume, the second to appear, maintains the high standards set by Wirth's, published in 1977. Pernambuco is located just beneath the "hump" of Brazil and is one of the oldest regions of continuous plantation agriculture in the world, having produced sugar since the 1530s. Its character even today is influenced by social and economic arrangements forged in the sixteenth century: concentrated land tenure, slavery, plantocracy, and dependence on exports. From the eighteenth century on, it became a backwater left by the southward tide of people, enterprise, and progress. Levine says that the most enduring quality of *pernambucanos* is their tenacity, but that the traditions to which they have clung are anachronistic. As they entered the twentieth century some colonial institutions withered and disappeared, but the economic and political decadence persisted. Pernambuco remained immune to most of the processes of modernization; emancipation of the slaves, industrialization of sugar refining, better communications, and railroads and motorized transportation brought neither crisis nor prosperity to the region. No wonder, then, that the narrative has a tone of unrelieved pessimism.

The book has no heroes, but neither has it villains. Levine describes Pernambuco's history as "history *malgré soi*"—largely the result of external events and pressures. Yet even outsiders are not fully to blame. Although he speaks of the state as a satellite of the federal government and notes penetration by foreign investors, Levine does not explicitly endorse a "dependency" analysis of Pernambuco. Likewise he dismisses as minor the state's control over its own smaller neighbors. Probably because of his unwillingness to provide a simple explanation for the state's backwardness, Levine's conclusion lacks punch. He implies that even in the 1970s things remain largely unchanged.

A reviewer is obliged to note flaws, though in this case they are minor. Although the topical

organization is straightforward, the author occasionally repeats material or strays from his subject. At other times he relies on the outline to give coherence to the narrative, failing to provide analysis at key points, such as the introductions to chapters two and six. A glossary might have been in order for nonspecialist readers.

Certainly the scope, innovative methods, and results of the regional project are among the most exciting developments in recent Latin American historiography. Levine's book will be of great benefit to Latin Americanists and social scientists in general for its solid contributions to Brazilian history and to regional studies of the Third World.

MICHAEL L. CONNIFF
University of New Mexico

JOHN HEMMING. *Red Gold: The Conquest of the Brazilian Indians*. Cambridge: Harvard University Press. 1978. Pp. xvii, 677. \$18.50.

Educated guesses place the Brazilian Indian population in 1500 at 2,431,000 and today at 100,000. Intervening centuries have witnessed a massive demographic, humanitarian, and cultural tragedy. Native peoples who inspired Erasmus's *In Praise of Folly* and More's *Utopia*, and who provided food for thought to Montaigne, have been all but destroyed. This holocaust has found its chronicler in John Hemming who brings to the task impeccable credentials as explorer and scholar (*The Conquest of the Incas* [1970]). As a member of the team sent by the Aborigines Protection Society to report on the status of Brazilian Indians and as contributor to the creation of Survival International, Hemming's own allegiances are clear. *Red Gold* is the first of a projected two-volume work intended to heighten awareness of the historical precedents for the present plight of the Brazilian Indian.

Red Gold is the history of the genocide wrought on Brazilian Indians, intentionally and unintentionally, by Europeans. On first contact the Indians' fascination with new tools paralleled the delight of the Portuguese at the innocence, physical beauty, and moral qualities of the Indians. But initial harmony soon gave way to disillusionment. Natives were disenchanted by increasingly brutal European behavior and the Portuguese were disappointed once they realized the limited value of Indians as sources of labor. Transition from barter to slavery was inevitable, given the irreconcilable points of conflict—Portuguese demands for labor and Indian satiation with fish-hooks, lengths of cloth, and tools. From this point the history is one of unceasing conflict. Portuguese offensives, slaving expeditions, and reprisals were thinly disguised under the legalistic euphemisms of "just

wars," "ransom expeditions," and "descents." From the Negro to the Uruguay and from Bahia to Mato Grosso tribes were systematically destroyed. Governors led expeditions and Jesuits accompanied flotillas of canoes that "descended" tribes purportedly for the salvation of souls but in reality for the capture of slaves. Nomadic and headstrong Paulistas raided Jesuit villages with impunity and openly attacked and slaughtered Indians. Indians were drawn into rivalries between the French, Portuguese, and Dutch; they discovered all too late that only disaster ever followed the defeat of their erstwhile allies. The Indians also fell victims to European diplomacy as when Indian mission villages of the Amazon and east of the Uruguay were disrupted after the Treaty of Madrid. If not destroyed by forced labor or war, tribes were decimated and even annihilated by measles, smallpox, and the common cold.

Genocide was accompanied by ethnocide. The superficial simplicity of Indian villages was deceptive. Complex rules governed behavior, labor, kinship, courting practices, and marriage. Mythology, religion, artistic expression, and, in some regions, trade were highly developed. All these practices were undermined and eventually destroyed by the Portuguese. Even the Jesuits, whose *aldeias* represented the most enduring attempt at coexistence, failed to appreciate the complexity of Indian culture. Tribes who had been "descended" were thrown together with little regard for linguistic and cultural differences. This system, coupled with dawn-to-dusk regimentation, resulted in detribalization and the creation of an *aldeia* subculture.

Little was done to prevent this destruction of human life and tribal culture. The problem of how to reconcile settler demands with Indian rights remained insoluble. Crown legislation was incoherent and ambiguous. Governors either failed to enact legislation or did so halfheartedly. By 1755 the Indian population was too depleted and too ill-prepared to benefit from the Pombaline Law of Liberties.

Hemming is far too good a historian to underestimate the achievements of the Portuguese in their struggle against a hostile environment, European rivals, and the indigenous population. The Jesuits earn his respect because of the determination, self-discipline, and bravery that enabled them to forge a chain of missions from the Bolivian *altiplano* to the Atlantic. Even the Paulistas—scourge of Jesuit and Indian alike—command admiration for their epic deeds and travels. Yet one aspect of European-Indian contact—the manipulation and exploitation of Europeans by Indians—emerges only obliquely from this account. Indians played off European rivalries to further their own

ends against traditional enemies and they proved inconstant allies to Europeans, changing sides depending on the fortunes of battle or once tribal ends had been achieved. The missionaries' disillusionment, although born in part of their own exaggerated anticipation of Indian pliability, resulted no less from their realization that they had been manipulated by Indians in search of material gain.

In the absence of a satisfactory history of the colony in English, the author has not presumed any knowledge of Portuguese America by his readers. This has forced him to supply background material on European activities that sometimes intrudes upon the major theme. It should be emphasized that *Red Gold* is not "the vision of the vanquished" or a history of the conquest as witnessed and recorded by Indians. Despite heavy use of contemporaneous accounts, some of which are sympathetic to the Indian position, this is the history as told by Europeans for a European audience. Hemming has also faced a problem with his sources, which are incomplete for some periods and regions. Events for which there is extensive documentation are described in great detail, but no less important events (for example, the displacement of Indians in the northeast to provide grazing) receive only general treatment. The same is true for particular tribes. Hemming has had to rely largely on accounts by non-Portuguese writers for, with few exceptions, the Portuguese showed little interest in ethnography. As a result of these problems, the narrative has an uneven quality. Hemming uses extensive quotations from his sources and, although the excerpts are sometimes illuminating, their use occasionally disrupts the narrative and leaves the impression of an anthol-

ogy. Finally, a problem arises from the theme itself; catastrophe thrills initially but palls rapidly. In part Hemming has offset monotony by his attention to individuals whose exploits capture the imagination.

Red Gold is a book that no one can afford to ignore. For the student of comparative colonialism, culture contact, or Brazilian history it is both a succinct account of the conquest of the Brazilian Indians and an invaluable reference work. Bringing firsthand experience to his subject, Hemming has succeeded brilliantly in describing clearly and coherently the complexities of classification and mythology. The story is compelling as a narrative of human endeavor and personal courage by Indian and Portuguese alike, each convinced of his own superiority in the fight to preserve religious beliefs and traditional values and to defend land and family. But *Red Gold* is more than merely a "good read." Alcohol and miscegenation continue to be instruments of domination, and the Indians of Brazil are not alone in failing to play a role in the new societies created on their traditional homelands. The passage of time only makes more acute the need to answer the moral question of whether tribal peoples should be integrated or permitted to remain isolated from so-called civilization. Hemming's sensitive work may create an awareness, permitting other native peoples to be spared the fate of the Brazilian Indian. Ringing in our ears across time and space is the heart-chilling reply made to the German Jesuit João Felipe Betendorf by the Taconyape, "We want none of your 'mercy'!"

A. J. R. RUSSELL-WOOD
Johns Hopkins University

Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

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Communications

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TO THE EDITOR:

I am writing in reference to the interesting essay by Roger M. Anders, "The Rosenberg Case Revisited: The Greenglass Testimony and the Protection of Atomic Secrets" (*AHR*, 83 [1978]: 388-400). Anders raised the issue of the guilt or innocence of the Rosenbergs and linked this question to a determination of the veracity of the testimony of David Greenglass, implying that, if Greenglass's knowledge of the bomb was correct, then he was a credible witness. Indeed, his piece concludes on this very note. Unfortunately for his argument, which is very persuasive, the case against the Rosenbergs did not turn on how much Greenglass knew about the design of the atom bomb (he could have been its inventor); the only question was whether he had acted on the orders of Julius and Ethel Rosenberg. The Rosenbergs denied that this was the case; Ruth and David Greenglass asserted that this was true.

The only courtroom testimony linking the Ro-

senbergs to Greenglass was provided by Harry Gold. "I come from Julius" and the jello box became the symbols of the great conspiracy. Greenglass's description of the bomb was elicited only to prove that he could have passed certain sensitive information to somebody else. But was it passed to Harry Gold? Did Harry Gold really "come from Julius"? Nothing in David Greenglass's unsubstantiated testimony could prove these latter points, and we now know that the FBI went to great lengths to "match" the testimony of Gold and Greenglass in pretrial interviews and discussions. Consequently, to demonstrate that Greenglass knew about the bomb as *prima facie* evidence of his overall credibility is seriously to distort the context and development of the Rosenberg case.

The article by Anders does, however, raise another point. Just prior to the execution of the Rosenbergs, Attorney General Brownell (in arguing against executive clemency) told President Eisenhower that the government withheld evidence from the trial that would have implicated the Rosenbergs much more deeply. Could he have been referring to the AEC files? To give the devil his due, perhaps the activity of the AEC helped persuade government prosecutors that a strong case should be built even if they had only weak evidence at their disposal. In any case, it is now clear that the Rosenberg case provoked enormous behind-the-scenes activity by many government agencies, activity that was barely noticed at the time but that shaped the trial and continues to provide substantive grounds for questioning the verdict.

MICHAEL WEISSER
*University of South Carolina,
Columbia*

TO THE EDITOR:

Roger M. Anders's essay, "The Rosenberg Case Revisited: The Greenglass Testimony and the Protection of Atomic Secrets," contains two inaccurate references to our book, *Invitation to an Inquest* (1965; Penguin, 1973).

In discussing David Greenglass's description of the Nagasaki bomb, Anders has written that "Walter and Miriam Schneir state that Greenglass fabricated his testimony with the help of Harry Gold. See . . . Schneir and Schneir, *Invitation to an Inquest* (New York, 1965), 416-17. There is no indication of such fabrication in Atomic Energy Commission documents. An examination of the statement of Harry Gold before Myles J. Lane and James G. Beckerley, March 12, 1951, and of Gold's testimony at the Rosenberg trial indicates that Gold knew very little about the technology of the atomic bomb" (n. 5). With this powerful fusillade, Anders has shot down a phantom. Nowhere in our book did we suggest that Greenglass's atomic bomb testimony was "fabricated" or produced "with the help of Harry Gold." Our position is that Greenglass's very limited technical knowledge of the Los Alamos project was consistent with his wartime assignment there and his background as a high school-educated machinist; his expertise neither proves nor disproves his espionage allegations.

Elsewhere and somewhat contradictorily, Anders stated that "Walter and Miriam Schneir feel that Greenglass pieced his description of the bomb together after the bombing of Hiroshima and Nagasaki from a combination of gossip, newspaper reports, and the Smyth report; see *Invitation to an Inquest*, 274. But, in fact, Greenglass got his information during the construction of the Nagasaki bomb and gathered it by observation, inquiry, and deduction" (n. 11). Here, again, Anders has created a straw man. We never said that Greenglass obtained his information *only* from "gossip, newspaper reports, and the Smyth report." Nor *only* after the bombing. The fact is that we do not know when Greenglass acquired each of the pieces of his fragmentary knowledge of the Los Alamos project. But Anders apparently does. He unequivocally stated that "Greenglass got his information during the construction of the Nagasaki bomb." As evidence, he offered a chart prepared in 1951 by AEC officials after an interrogation of Greenglass. But the chart provides no such proof. It contains no dates.

The subject of the AEC's involvement in the Rosenberg case is certainly worthy of attention by historians. But it is more than a technical matter. A useful analysis will require consideration of the political context. Ignoring this, Anders concentrated on minutiae and omitted the broader question regarding the role played by the AEC in one of the big propaganda lies of American history: that the Greenglass data allegedly passed to the Russians by the Rosenbergs was of such awesome importance that, according to Judge Kaufman, it "caused . . . the Communist aggression in Korea . . . [and] altered the course of history to the dis-

advantage of our country" and, according to President Eisenhower, it "may have condemned to death tens of millions of innocent people all over the world."

WALTER SCHNEIR and
MIRIAM SCHNEIR
Pleasantville, New York

MR. ANDERS REPLIES:

My essay, "The Rosenberg Case Revisited: The Greenglass Testimony and the Protection of Atomic Secrets," which explores the role of the U.S. Atomic Energy Commission in the Rosenberg case, states that the prosecution of the Rosenbergs presented the commission with a major security problem. In order for the Department of Justice to obtain a conviction, it appeared necessary for the AEC to allow David Greenglass to reveal in open court previously classified atomic bomb technology. Nevertheless, the AEC worked to minimize the amount of technical data released during the trial. I concluded that the commission successfully solved the security problem, protecting most of the technical information that it wished to keep secret. Underlying the commission's action in the case was Greenglass's veracity about atomic bomb technology. In my conclusions, therefore, I also observed that David Greenglass, who was generally truthful regarding weapons technology, had acquired technical data of some sensitivity. I did not address the question of the Rosenbergs' guilt or innocence because that issue went beyond the scope of my essay and beyond the evidence available from the AEC files.

The commission did not work in a political vacuum while attempting to solve the security problems presented by the Rosenberg case. The AEC and the Department of Justice kept the Congressional Joint Committee on Atomic Energy informed of the government's prosecution of the case, including precautions to limit the release of technical information. Although the joint committee believed that the Rosenbergs had to be prosecuted, it concurred in minimizing the release of technical data. When Bourke B. Hickenlooper of Iowa, a Republican member of the committee, learned that federal prosecutors might not exercise care in divulging technical data, he put pressure on the AEC to limit disclosures.

Turning to specific comments on my article, Walter and Miriam Schneir claim that I misrepresented them, ignored the political context of the case, and concentrated on "minutiae" rather than "broad questions." With respect to the alleged misrepresentation, readers should examine the Schneirs' *Invitation to an Inquest* (Penguin edition, 1973), pages 274 and 416-17, and draw their

own conclusions. I have discussed the direct political pressure placed on the Atomic Energy Commission during the Rosenberg case; I did not, however, discuss the political climate of America in the early 1950s because such a discussion is beyond the scope of a short essay. Finally, what the Schneirs characterize as "minutiae" are historical facts gleaned from primary sources and conclusions carefully based on that evidence; indeed, on that basis I have attempted accurately to portray the broad role of the AEC in the Rosenberg case. Unfortunately, the Schneirs dismiss as merely "technical" my efforts to treat the Rosenberg case as a subject for scholarly inquiry rather than as a vehicle for propaganda.

In his letter Michael Weisser first argues that to demonstrate that David Greenglass was telling the truth about the atomic bomb is not to show that Greenglass was telling the truth about the Rosenbergs' guilt. Naturally I agree that Greenglass's veracity about the bomb does not establish his overall credibility as a witness; and I so stated: "Did David Greenglass tell the truth? The records of the Atomic Energy Commission only tell us that the technological details about the atomic bomb in his courtroom testimony were fairly accurate" (p. 400). But Greenglass's veracity in this instance is significant because Greenglass's testimony has been dismissed as a fabrication in some studies of the Rosenberg case. Such an evaluation is not accurate, and I hope my essay has contributed to a more realistic portrait of David Greenglass as a witness. Weisser then asks whether the commission provided the evidence when Attorney General Brownell argued against executive clemency for the Rosenbergs. The complete answer is beyond the scope of my research and the commission files. I do not know what technical or legal evidence Brownell cited to Eisenhower; I saw nothing in my research to indicate that Brownell obtained evidence directly from the AEC.

Finally, Weisser suggests that the commission stimulated federal prosecutors to build a strong case against the Rosenbergs out of weak evidence. Quite the contrary, as my article shows, the AEC's role inhibited federal prosecutors because the commission was trying to protect atomic bomb technology. To accomplish this, the commission told federal prosecutors what to ask Greenglass as well as the expert witnesses, Walter Koski and John Derry, on direct examination. As the commission chairman, Gordon Dean, told the Department of Justice, it was imperative to minimize the weapons data that might be revealed in court. Minimizing the weapons data tended to weaken, rather than to strengthen, the government's case.

ROGER M. ANDERS
Department of Energy

TO THE EDITOR:

I should like to venture a few comments on the article entitled "The Character of Erasmus" by Nelson Minnich and W. W. Meissner (*AHR*, 83 [1978]: 598-624). The authors frankly acknowledged that they did not restrict themselves to a cautious historical investigation but sought "a clear and coherent psychoanalytic interpretation of Erasmus' character" (p. 622). That admission is puzzling and, in my estimation, hardly acceptable to the serious historian; and their analysis, I think, clearly demonstrates the danger of their approach. Let me put it bluntly. The grounds for their interpretation are insufficient, and their interpretation itself is highly tendentious. Despite a considerable amount of footnoting, one senses almost immediately an inadequate acquaintance with the Erasmian corpus, and one perceives an acceptance of the view of Erasmus, or part of the view, that some latter-day historians—notably Johan Huizinga and John Joseph Mangan—have given us. Thus, to one fairly familiar with the writings and work of Erasmus and with the historiographical traditions of interpreting him, the Minnich-Meissner presentation lacks the authority that for some its psychoanalytic approach and terminology might bestow upon it.

And how should one judge their "clinical interpretation" if not on the basis of the historical evidence? The historian is naturally reluctant to turn over his profession to the psychiatrist or accept psychoanalysis as higher revelation. This reluctance does not mean that the historian refuses to make use of other skills or insights, but it does mean that historical understanding rests primarily on the conscientious and informed examination of the evidence. Many of the pronouncements in Minnich and Meissner's article will certainly strengthen historians' fidelity to their craft. To speak, for example, of Erasmus' enormous literary and scholarly labors—that is, his life's work—as a huge effort to justify himself and overcome an "injured narcissism" (pp. 622-24) is absurd or, at best, an exceedingly narrow and irrelevant "clinical" observation. One need only study the *Adagia*, which contains so many marvelous essays and which had so many enlargements and editions in Erasmus' lifetime, or examine the great edition of Saint Jerome, over which Erasmus labored with such devotion and dedication and in which, among other things, the first critical biography of the saint appeared, to realize how misplaced and inaccurate and, indeed, unworthy the judgment of Minnich and Meissner is.

There are several other features of the Minnich-Meissner analysis to which one might also take exception, such as their understanding of the *Praise of Folly*, their view of Erasmus' friendship with

Thomas More, and their notion (similar to that of Huizinga) of Erasmus' indecision and ambivalence. The *Praise of Folly* is a controversial book, but it is not "the most uncharacteristic of his works" (p. 608), whatever that means, nor is it in any notable sense an "elaborate self-parody" (p. 608). I suggest that the view of More as a "major father figure" (p. 610) for Erasmus is nonsense and damaging to any appreciation of the substantial bond of friendship that did exist between the two humanists. As for the charge of indecision and ambivalence, the allegation has to be considered in the full context of Erasmus' goals and labors. It seems to me that Erasmus did have clear aims, firm convictions, and high ideals and that he held to them with remarkable consistency throughout his life.

While still on specific points, I also want to comment on the curious "speculation" of the authors regarding Erasmus' attraction to humanism and aversion to scholasticism (pp. 604-06). They have indicated that humanism satisfied certain needs of his disturbed personality and "allowed him to escape his origins, avoid entangling loyalties, and sink few roots" (why John O'Malley's article on Erasmus and Luther in the *Sixteenth-Century Journal*, 5 [1974], is footnoted here is unclear), whereas he found scholasticism "too difficult" and thus an affront to his self-esteem. Undoubtedly, personal inclinations and motives play a role in our intellectual choices, but to personalize Erasmus' "decision" on this important issue to such an extent is a gross and misleading exaggeration. Humanism had its own inherent appeal and was beginning to sweep all of Europe in the latter part of the fifteenth century. Many other scholars besides Erasmus were won enthusiastically to the cause, and one may suppose that broad cultural factors as well as specific historical circumstances were operating. But that aside, the emphasis on the "difficulty" of scholastic studies is a novel and unexpected note to strike in explaining Erasmus' attitude. That attitude had deeper and firmer footing, just as his frequent criticism of scholastic theology had a serious reform purpose. Anthony Levi's introduction to the Penguin edition of *Praise of Folly*, to which the authors referred (in addition to O'Malley's article), should have made that point clear.

The problem with analyses such as appear in this article is not simply that they are inadequately grounded and narrowly conceived but that they drain a person's aims and efforts and life itself of any real meaning. What one carries away from the article is that Erasmus was a pathetically troubled and sick individual whose "character" was damaged at the outset by his rejection by his father. The picture is unworthy of so great a man, and,

what is more, it is untrue. Indeed, it smacks of the kind of attack that rival scholastic theologians launched against him in his own day, save that a Freudian garb now cloaks the rancor and dismay of his adversaries. The correspondence between old and new critics may be closer than one thinks, for in both interpretations a rigid methodology, an arcane terminology, and a concern with what proves to be irrelevant tend to prevail. The Freudians, it would appear, have grasped the cudgels from the faltering hands of the late medieval scholastics. But not every psychologist or psychiatrist, I believe, would arrive at the same diagnosis. I recall seeing a film of Carl Jung a few years ago on television. He was being interviewed in his study, and on the wall in back of where he sat hung a reproduction of Holbein's great portrait of Erasmus at the Louvre. That was not an accidental display, and I am certain that Jung had a very different opinion of the famous scholar and Christian humanist from that held by the authors of this article.

JOHN C. OLIN
Fordham University

PROFESSOR MINNICH AND DR. MEISSNER REPLY:

The criticism of our interpretation in "The Character of Erasmus" claims that it is "inadequately grounded," "highly tendentious," and unfairly hostile. Our claim that we "sought 'a clear and coherent psychoanalytic interpretation'" does not, when read in context, amount to a frank confession of our having abandoned the canons of historical evidence. The hypotheses that the psychoanalyst bring to the study of character are not a "higher revelation" but are derived empirically from an analysis of multiple cases. Their applicability to a particular historical figure can be determined only by establishing the similarity between a clinically verified pattern of thought and behavior and the historically documented words and actions of the individual in question. Given this approach, our findings were in most cases proffered only as reasonable hypotheses. Our methodology has, therefore, been rigidly bound by a respect for historical facts: the historian's role has been to establish and interpret them in their historical context, the psychoanalyst has suggested various patterns of intelligibility into which they can be arranged, and, by a constant recourse to the historical record, both have tried to determine which psychoanalytic hypothesis is most in keeping with what is known about the subject and his times.

That our acquaintance with the Erasmian corpus is inadequate to our purpose is a charge difficult to answer. Because we did not cite a specific

work does not mean we are unacquainted with it. What relevant writings were neglected? Closer attention to his editions of many of the Church Fathers may have documented more fully his dedication to a reform of theology based on ancient Christian writings, but it is doubtful that this closer attention would have brought to light psychologically relevant materials that would notably have altered our interpretation. A dozen or so significant writings, from his youthful *Antibarbari* to the *Liber de sarcienda ecclesia* written in his old age, were used in describing different aspects of Erasmus' character. Preference, however, was given to his letters, since these record his attitudes toward a host of important figures and events in his life. Specific references were given to well over one hundred of these letters, covering almost fifty years of his life. We would appreciate learning which writings of Erasmus contradict the data derived from the materials we used.

To claim as grounds for dismissal that our interpretation of Erasmus is too dependent on those of Huizinga and Mangan is to suggest a lack of independent judgment and to ignore the more important issue: are these interpretations substantiated by the historical record? The similarities of interpretation, we maintain, are due to some of the same or related evidence in the Erasmian corpus that we have all used. In spite of a half-century of Erasmian scholarship that has mandated a number of repairs, Huizinga's portrait still remains in our opinion the most perceptive study of his character. With what more recent work should it be replaced?

The criticism on a number of specific points is equally tendentious. If the criticism of our description of the *Praise of Folly* as the most uncharacteristic of Erasmus' works and an elaborate self-parody is in error because many similar themes and treatments appear in his other works, notably his *Colloquies*, we are in agreement. But *Praise of Folly* is "uncharacteristic" in that no previous work was so playful, so biting, and so sweeping in its attacks on human vanity. Erasmus was later apologetic for its publication and never again produced a similar work of such sustained artful satire and literary merit. *Praise of Folly* can also be considered special in that Erasmus attacked the burdens of learning and the fears and shames that weighed upon his spirit, and thus it deserves to be seen as a piece of elaborate self-parody unlike any other he wrote.

It is not at all clear to us that his *Adages* and works on Saint Jerome's life and writings disprove the contention that his scholarly labors were an attempt to justify himself (p. 621). By the term self-justification we mean the effort to prove or promote the worth of one's self-conception, position, or deeply held ideas. The *Adages* were not an anon-

ymous manual of sayings with an impersonal scholarly commentary but a carefully selected collection of proverbs that provided Erasmus with the opportunity to display his erudition, comment on his times with personal reminiscences and ironic indignation, and advance his own reform ideas. His decision to "restore" Saint Jerome, instead of some obscure and neglected Church Father, was not determined by some impartial criterion but was dictated, at least in part, by personal considerations. Erasmus felt a special affinity for Saint Jerome due to their shared intellectual interests. By writing a life of the saint and republishing his works in an improved edition, Erasmus was propagating ideas similar to his own, promoting the cause of "good letters" with which he intensely identified, and garnering the acclaim of his learned colleagues.

That Erasmus' relationship with Thomas More may have been determined to some extent by unconscious factors does not seem unreasonable, nor need it be viewed as damaging to an appreciation of their friendship. We are merely suggesting that the search for paternal affection and approbation may help explain why More, who embodied so many qualities of an idealized father, held a special place among Erasmus' friends. We do not deny that other important factors were also present. And we agree that Erasmus had high ideals that he fervently hoped would be realized. His ambivalence and indecision primarily affected the course of action he was willing to take in order to realize these ideals, especially when controversy might ensue. His repeated declarations of subscription to or sympathy with a circumscribed skeptical stance cannot be ignored, however, in assessing the firmness with which he held to his ideals.

Although our article concentrates on facets of Erasmus' personality that probably went into his decision to pursue the career of a humanist, it does not ignore broader cultural influences, such as those present during his training at Deventer (p. 604) and visits to England (p. 607). Our recourse to passing references rather than extended passages describing the humanist culture of his day is attributable to an assumed familiarity of the reader with this feature of the Renaissance and to the revisions mandated by length restrictions. We recognize that a comprehensive understanding of Erasmus must also consider such factors as the structures of his thought and modes of expression shaped by classical models. As interesting as these and other questions may be, we have tried to limit ourselves to a study of his character. O'Malley's article gives a useful summary of some of the common humanistic elements in Erasmus' outlook: the use of ambiguity in the pursuit of eloquent per-

suation, an avoidance of the clear concepts and sharp distinctions of the scholastics, a quest for peace, harmony, and concord, and preference for theology's function of encouraging Christian living over its rational analysis of doctrines. Our article speculates on why this humanistic outlook should have proven so attractive to Erasmus.

The reasons for Erasmus' opposition to scholasticism are many. Our brief summary of some of his major objections (p. 606) is not dissimilar to that of O'Malley ("Erasmus and Luther: Continuity and Discontinuity as Key to Their Conflict," 52-53), and we explicitly acknowledge a "significant kernel of truth" in Erasmus' complaints. The influence on him of an early practitioner of the "new theology," the different methodology it involved, and the ultimate goal of its study were not neglected in our article (pp. 607, 621). That Erasmus found scholasticism difficult and never mastered what he criticized is not a novel observation. It is commonly known that he never earned his doctorate at Paris, where he spent time as a student, but received it at Turin, on the basis of his claims to have completed the requirements elsewhere. Why did he settle for a degree that lacked the prestige of one from the Sorbonne and to which he had a dubious title? The research of McSorley has shown how deficient Erasmus' understanding of scholasticism really was. Is it surprising, therefore, to suspect that personal difficulties with the study of scholasticism played a significant role in his intense dislike of it?

To see our article as "hostile," as an attack on the well-earned fame of Erasmus is to misread its argument and attribute to its authors a set of intentions we never had. If an investigation into his character has revealed certain patterns of thought and behavior that make him more understandable as an imperfect man rather than as a demi-god, we do not apologize. Personal problems do not detract from one's greatness. Instead of rancor, we hope our study has led to new avenues of understanding; instead of dismay, admiration for Erasmus' achievements and a recognition that problems need not impair genius but can also serve as its prod. Although we paid recognition to the pioneering insights of Freud, the article's approach is eclectic. Our sketch of Erasmus' character was advanced as a reasonable hypothesis and the possibility of other interpretations openly acknowledged (p. 622). We welcome a Jungian study with the expectation that it will contribute significantly to the study of Erasmus' character. The terminology used in our article was carefully chosen to be readily understood by most educated persons. Concepts such as father-figure, identification, narcissism, and paranoid tendencies we thought would not be considered arcane by most

readers, nor do these terms merit the label "jargon" when appearing in a scholarly psychoanalytic study. We are curious as to what elements in our portrait of Erasmus deserve to be typified as irrelevant and what factors pertinent to a discussion of his character have been ignored. No doubt there are some, but a mere twenty-eight page article that attempts to trace the outlines of a major figure's life, analyze his character, and provide methodological discussion at key points cannot say everything.

We do appreciate, however, the thought-provoking observations on our article.

NELSON H. MINNICH

Catholic University of America, and

W. W. MEISSNER

Harvard University Medical School

TO THE EDITOR:

Permit me to clarify a few points in Lawrence Schofer's review of my book, *While Messiah Tarried: Jewish Socialist Movements, 1871-1917* (AHR, 83 [1978]: 973-74). The date 1917, to which Schofer objected, obviously does not cover all of the bases of Jewish socialism; the Bund in Poland did indeed flourish, and there was still some vitality in the American Jewish socialist movement. But, since the book uses tsarist Russia as the time-place in which the three most important movements (in America, Russia, and Palestine) had their origins, I limited myself to those movements. The Bund in Poland had a very different history from the Bund in Russia, and the Bolshevik Revolution—1917—marked the beginning of the end of the Russian Bund. In America, the Jewish socialist subculture, which had had a fairly well-defined character and thrust up to World War I, underwent great changes afterward. As I wrote, "After World War I, there was a fast deproletarianization of Jewish workers and movement into the middle class. The earlier style of life declined, even though certain unions still contained many Jewish workers and leaders. . . . Within one generation, many immigrant workers—and certainly their children—had climbed into the middle class, depleting the ranks of Jewish socialists and dissolving the once homogeneous, identifiable Jewish socialist society." The tension between utopian vision and adaptation to America, which the "Marxist corset" had once been able to resolve, tended to break under middle-class aspirations and compromises. Moreover, the brief, heady enthusiasm over the Bolshevik Revolution was soon embittered by the swift elimination of all political opposition in Russia, the relentless Communist drive to capture the Jewish unions in the garment industry, and the split in the Socialist International. These points are elaborated on pages 208-15.

As to Schofer's question—"What did it [the Bund] mean as a social movement?"—following his statement that it is "often difficult to know just what the Bund meant beyond the infighting at the very top levels of the parties," I can only say, "Alas, it is obvious that he has not read my book." The achievements of the Russian Bund were immense, making possible the first important politicizing of Jewish workers, their first large steps into the modern secular world, into a sense of self-worth and dignity and the values of social struggle. The Bund was a movement that uniquely linked Jewish intellectuals with workers and also gave Jewish workers a sense of solidarity with non-Jewish workers—even after Lenin expelled the Bund from the Russian party. As to the struggle with Lenin, it signified far more than "infighting" or "a battle of party leaders." The clash was over fundamental issues: the need to acknowledge anti-Semitism as a problem unique to Jewish workers, and express the *cultural* identity of Jewish workers, a position that led to the Bundist formulation of cultural-national autonomy—scarcely an unimportant contribution. Thousands of Jews were also served by the Bundist press, libraries, and schools as well as social welfare services.

I agree—the price of the hardback is high, but in March Schocken is bringing out a paperback (\$10.95) for interested readers.

NORA LEVIN
Gratz College

PROFESSOR SCHOFFER REPLIES:

I would guess that almost every author of a reviewed book has made some comment like, "Alas, it is obvious that the reviewer has not read my book." I can sympathize with the author's frustration, but that does not alter my perception of what the book *actually* says. Interested readers can turn to the text themselves (although at \$10.95 for a paperback it is likely that they will wait for the library's copy).

My primary objection to this work lay in the lack of a convincing framework for the study, a lack apparent in the choice of 1917 as a terminal point. Many historians fasten on some handy date as a way of organizing their material, and 1917 is quite popular for the purpose. Such a convenient choice, however, leads to a warping of historical context and an emphasizing of less important developments at the expense of more important changes. Jewish socialism is a fascinating topic, but one can hardly grasp its essence in Russia, America, and Palestine by breaking off at 1917. Levin's period, 1871–1917, contained the beginnings of some of these developments, but these roots grow all out of proportion when considered

alone. Of course, the Bund made possible the first steps of Jewish workers into the modern secular world, but where did those steps lead? Those steps were often taken in the territory that would become interwar Poland, and only from that later vantage-point can one evaluate what happened earlier. In Palestine even the "roots" were very faint before the 1920s, and I suggest that to say that Jews in America were deproletarianized and of middle-class outlook after World War I is an assertion without convincing evidence. Embourgeoisement does not occur overnight, and it took more than the magic date 1917 to wipe out socialism among American Jews.

In sum, I objected not so much to what Levin said in her book; her summary of events is perfectly adequate. I emphasized instead her failure to come to grips with one of the major problems a historian must face—What are the bounds of the subject? Is the intellectual construct portraying historical change a meaningful one? The answer, I submit, is that this project is unsatisfactory from this point of view.

LAWRENCE SCHOFFER
University of Pennsylvania

TO THE EDITOR:

Thank you very much for the attention you have given my book, *Danish Neutrality during the Crimean War (1853–1856): Denmark between the Hammer and the Anvil*, by publishing a review by Leland B. Sather (*AHR*, 83 [1978]: 1275). I read the review with great interest, and I am grateful to the reviewer for it. I must, however, disagree with his assertion that the book has "some serious shortcomings" since I feel that the points he has raised either reflect, gently speaking, a lack of accuracy on his part or are problems open to discussion. I was rather surprised to read that the book does not define "clearly the particular predilections that the A. S. Ørsted–C. A. Bluhme ministry possessed regarding foreign policy" and that "the characteristics of the Danish government led by P. C. Bang and L. N. Scheele . . . are equally unclear." It is, in fact, the essential subject of my book presented almost on every page of my work and especially on pages 16, 17, 136, 182, and 183, and I cannot understand why the reviewer did not notice it.

As to the military activities, I stated clearly in my book that these did not have a great impact on Denmark's policy. I treated this problem as it deserved—dealing in detail with the problem of the appearance of Admiral Napier's fleet in the Baltic, which caused considerable interest in the spring of 1854, and in general terms only with the second Baltic campaign of 1855, which did not influence Danish politics at all. For this reason

there was no further need to summarize military activities in the Baltic.

Especially surprising was the demand to consider in detail Swedish foreign policy during the war. It should have been clear that the subject of my book is not *Scandinavian* but *Danish* neutrality during the Crimean War. I did, in fact, consider the Swedish problems, but only in so far as they were connected with Danish neutrality, especially in 1853 and 1855–56. This would seem to be a reasonable approach given the basic subject of the book as indicated by the title.

It is hard to understand how the reviewer could have stated, "nor does he [the author] mention frequent Swedish negotiations to obtain an alliance with Britain and France." It is, in fact, mentioned several times, for example, on pages 95, 152, 153, 222, and 232 and in connection with Canrobert's mission. I would be interested to know which aspects of Canrobert's mission (described in detail on pp. 164–71) deserve in the opinion of the reviewer "greater attention and clarification than this work provides."

In the context of a short letter there is no space to discuss the important methodological problem raised by the reviewer's objection to my use of contemporary opinion, especially when he referred to it in only the most general terms. I do not pretend to have solved all of the problems connected with Danish neutrality, but the shortcomings of my book suggested by the reviewer seem to indicate that he has not considered the subject with sufficient care.

EMANUEL HALICZ
Copenhagen

PROFESSOR SATHER REPLIES:

After reconsidering Emanuel Halicz's book in the light of his reply to my review, I still believe my original criticisms to be valid. Our differences seem to be mainly due to the methodology he chose to follow. As I emphasized, the author described Danish policy chiefly by paraphrasing foreign diplomats, Danish officials, and Copenhagen newspapers. As Halicz contends in his letter, contemporary opinion certainly has its place as a valuable historical resource. I feel, however, that the reader is too frequently told what might have happened or what was possibly the case but is not informed often enough of what actually occurred. This accounts, I believe, for many of the difficulties I noted in my review.

Finally, I wish to re-emphasize the constructive qualities of the book, as Halicz does treat Denmark's relations with Europe's minor powers very well. For this reason it can be said to contribute to our knowledge of Denmark and similar states dur-

ing the nineteenth century that were caught in events they were powerless to control.

LELAND B. SATHER
Weber State College

FROM THE EDITOR:

The April 1979 issue of the *Review* carried a letter from G. J. Barker-Benfield responding to a review of his book, *The Horrors of the Half-Known Life: Male Attitudes toward Women and Sexuality in Nineteenth-Century America*, which appeared in 1977 (*AHR*, 82 [1977]: 1328–29). In accordance with our standard procedure, we asked Linda Gordon, the reviewer, whether she wished to respond, and she declined. Since Barker-Benfield's letter contains certain allegations against Carroll Smith-Rosenberg and Charles Rosenberg, the editors should have asked both whether they wished to respond to the letter. By inadvertence we failed to take this step and have apologized to these scholars for this regrettable lapse. On our invitation, they have submitted the following responses to be published in this, the following issue.

OTTO PFLANZE

TO THE EDITOR:

In your April number G. J. Barker-Benfield in the course of commenting on a review of his recent work, *Horrors of the Half-Known Life: Male Attitudes toward Women and Sexuality in Nineteenth-Century America*, chose to accuse me and Carroll Smith-Rosenberg of a lack of originality and, by innuendo, of something akin to plagiarism (*AHR*, 84 [1979]: 615). These charges are based on my having looked at Barker-Benfield's UCLA dissertation in the winter of 1968–69.

Barker-Benfield specifically suggested that we gathered the idea of using nineteenth-century medical writings to study sexuality and sex roles from his thesis. Since I had already published two books and a number of other, shorter essays based largely on such medical materials by the fall of 1968, this struck me as a rather arbitrary supposition. In regard to matters of nineteenth-century sexuality, let me add in my defense that I had begun accumulating materials in 1960–61, when I held a postdoctoral fellowship at the Johns Hopkins Institute for the History of Medicine to study nineteenth-century ideas of heredity. Barker-Benfield has apparently assumed that I necessarily discovered the subject-area by reading his thesis—and dismissed the possibility that I asked to see his thesis because I was already interested in the subject. Inasmuch as Peter Cominos's influential interpretation of Victorian

uses of sexual metaphor, "Late Victorian Sexual Respectability and the Social System," appeared in the *International Review of Social History* in 1963 and Steven Marcus's widely reviewed and discussed *The Other Victorians: A Study of Sexuality and Pornography in Mid-Nineteenth-Century England* appeared in 1964, there seems little reason to assume that Barker-Benfield's thesis was necessarily the inspiration of all work published in the area after 1969.

Finally, let me conclude by noting that I find little similarity in Barker-Benfield's published views and mine, and I would hope that interested readers might make their own comparisons of the works in question and draw their own conclusions as to their respective merits and source of inspiration.

CHARLES E. ROSENBERG
University of Pennsylvania

TO THE EDITOR:

I feel I must first comment upon the editors of the *American Historical Review's* lack of professional courtesy in publishing such a vague and vitriolic

attack without informing the objects of that attack.

G. J. Barker-Benfield's letter (*AHR*, 84 [1979]: 615) is rather odd. He has carefully elaborated the frequency with which I have cited him in my varied articles and then failed to specify in any way what his influence upon my work has been, what ideas I have "borrowed" from him, what sources he has suggested to me. To read my work and Barker-Benfield's is to see the sharp differences between them. This letter, like so much of Barker-Benfield's rather histrionic writing, does not merit further comment.

CARROLL SMITH-ROSENBERG
University of Pennsylvania

ERRATUM:

The review of Carl V. Harris's *Political Power in Birmingham, 1871-1921* (*AHR*, 83 [1978]: 1353) incorrectly printed the title of a work by Floyd Hunter. The correct title should have been given as *Community Power Structure*. The *Review* regrets the error.

THE EDITORS

Recent Deaths

PAUL ALEXANDER died in Berkeley, California, on December 16, 1978. He had become Emeritus Professor only six months before, and his colleagues and students in the University of California, Berkeley, had warmly shared his own hope to continue teaching. One of the world's leading Byzantinists, he was, indeed, at the height of his career. He was engaged in studies of the medieval apocalyptic tradition which had already borne fruit in major publications—including "Medieval Apocalypses as Historical Sources" (*AHR*, 73 [1968]: 997–1018); and his international reputation, securely founded on earlier work in secular biography, patristics, and iconoclasm, had merited his election to numerous honorary fellowships, including the British Academy, the Istituto Siciliano di Studi Bizantini e Neoellenici, the Mediaeval Academy of America, and the American Academy of Arts and Sciences.

Like so many other scholars of his generation, Paul Alexander was a refugee from Nazi Germany. He was born in Berlin, where he attended the Französisches Gymnasium and received his first instruction in Greek and Latin. He took his early degrees in law, first at Hamburg (1932) on the eve of political disaster, then at Paris (1934). Working with Germaine Rouillard at the École Pratique des Hautes Études, he discovered his real vocation in Byzantine studies. Alexander came to the United States in 1935, pursuing work in Byzantine and ancient history first at Ann Arbor and then at Harvard, where he studied with W. S. Ferguson, J. H. Finley, and R. P. Blake and earned his Ph.D. in 1940. He was a Junior Fellow at the Dumbarton Oaks Research Laboratory from 1941 to 1943 and a Research Analyst in the Office of Strategic Services, before assuming his first teaching post at Hobart College in 1945. In 1954 he moved to Brandeis University, where he served for a time as chairman of the history department; in 1958 he became professor of history at Michigan, succeeding his former teacher A. E. R. Boak; and it was from Ann Arbor that he came to Berkeley, first as Visiting Professor in 1967, then as Professor of History and of Comparative Literature in 1968. In

addition to academic appointments Paul Alexander won research fellowships (including Guggenheim Fellowships in 1951 and 1965), membership in the Institute for Advanced Study (1951, 1970), and a Senior Fellowship of the National Endowment for the Humanities (1974). He enjoyed a long and happy marriage to Eleanor Eyck, who, as readers of this *Review* will recognize, is the daughter of a distinguished historian.

Paul Alexander was a scholar of extraordinary attainment. Thoroughly grounded in classical sources—literary as well as historical—he appreciated and taught the classics. He learned Syriac and Arabic as well as Old Church Slavonic and Russian. To this formidable linguistic equipment was added an uncompromising devotion to the study of texts and their transmission: characteristically, one of his last articles dealt with "A Neglected Palimpsest of Philo Judaeus" which he had discovered at Athens many years before in the course of research on Byzantine apocalypses. His first book was *The Patriarch Nicephorus of Constantinople: Ecclesiastical Policy and Image Worship in the Byzantine Empire* (1958); richly informed and judicious, it was recognized at once as the standard monograph on the later iconoclastic period (815–843). Two fine editions, demonstrating Alexander's mastery of codicology, appeared in the 1960s: the homilies of Saint Gregory of Nyssa (in Jaeger's distinguished *Gregorii Nysseni opera*, vol. 4 [1962] and *The Oracle of Baalbek: the Tiburtine Sibyl in Greek Dress* [1967]). The second of these grew out of the exciting research that Paul Alexander continued during his years at Berkeley preparatory to a major history of Byzantine apocalyptic literature. Substantial portions of this work will, it is hoped, be published posthumously.

Paul Alexander was as scrupulous in his teaching as in his research. He always began with the texts but never stopped short of appreciating the work of modern scholars. Rigorous and sincere in all he said and wrote, he made us understand that generosity and humanity were better than assertion and controversy. It was difficult to get him to dwell on his own work, impossible to hear him

speak ill of others. Erudite and disciplined, he was at ease with himself, good humored, a fine conversationalist, firm as well as gentle. As others who knew him well have observed, he was, above all, a "good man," one who expected the best of others as of himself. He gave richly of himself.

THOMAS N. BISSON
WILLIAM J. BOUWSMA
*University of California,
Berkeley*

DAGMAR HORNA-PERMAN died in Jerusalem on May 26, 1978, after a long illness. Born in Czechoslovakia, she was the daughter of a professor of constitutional law at Charles University in Prague. She emigrated to the United States in 1948, receiving her M.A. from the University of Kansas in 1949 and her Ph.D. from Berkeley in 1954. Dr. Horna-Perman's interests and accomplishments were many; she had the rare talent of excelling equally in research, writing, teaching, and service to the scholarly community. Her primary field was modern Czech history, and her study, *The Shaping of the Czechoslovak State* (1962), is considered a major contribution to that area.

She became interested in American history, through her work on the Charles and Richard Crane papers, and was working on a biography of Charles Crane at the time of her death.

As a young scholar, just out of school, she joined the AHA team in Alexandria, Virginia, that microfilmed and cataloged the captured German documents; she prepared and edited several guides to that important collection. Later, she became research director and editor of the AHA's joint committee on bibliographical services, in which capacity she organized the Belmont Conference in May 1967 and edited its papers (*Bibliography and the Historian* [1968]). In 1972 she was appointed to the history faculty at Georgetown University, and her lectures and seminars on German and Central European history quickly became popular and attracted many bright and loyal students. She was also interested in psychohistory, an interest she shared with her husband Gerald, a Washington psychiatrist. Together the Permans organized a psychohistory seminar for historians and psychiatrists through the Washington School of Psychiatry, and she later taught psychohistory at Georgetown.

In 1975 Professor Perman organized one of the most important recent U.S. conferences on modern German history, the Georgetown History Forum on "Sources and Interpretations in German History: the Weimar Republic and the Third Reich." The conference involved leading German and American scholars and was praised for its quality and breadth. The Georgetown Conference

best exemplifies Professor Perman's broad interests, many talents, and personal strength: her love of people and scholarship, and her facility for bringing the two together to achieve lasting and worthwhile results, despite great personal sacrifice.

Dagmar Horna-Perman had very high standards and excellent taste—she was also a fine writer, a beloved teacher, and a charming and courageous woman.

GEORGE O. KENT
*University of Maryland,
College Park*

ARTHUR RAYMOND KOOKER, Emeritus Professor of History at the University of Southern California, died on September 29, 1978, at the age of 71. Professor Kooker became a member of the USC history department in 1941. From 1955 to 1969 he chaired that department and was the driving force behind its rise to prominence in the profession.

A native of Michigan, Kooker received all three of his academic degrees, including his Ph.D. under the famed Dwight Dumond, at the University of Michigan. His dissertation was entitled "The Antislavery Movement in Michigan, 1796-1840, A Study in Humanitarianism on an American Frontier." Kooker's academic emphases focused upon the Civil War and Reconstruction and Franklin D. Roosevelt.

From 1942 to 1946 Kooker served in the Army Air Forces and was stationed at Santa Ana Army Air Base, Fort Worth Training Command. He wrote several sections, concerned with recruitment, in *Men and Planes*, volume six of *The Army Air Forces in World War II*. The seven volumes of this work were prepared under the editorship of Wesley Frank Craven of Princeton University and James Lea Cate of the University of Chicago and were published by the University of Chicago Press.

For years Ray Kooker was active in the Pacific Coast Branch of the American Historical Association, the History Guild of Southern California, and the Civil War Roundtable. In addition, he was a member of the American Historical Association, the Southern Historical Association, and the Society of American Archivists.

At the University of Southern California, Kooker served in several notable capacities: as president of the University Faculty Senate, faculty athletic representative to the Association of Western Universities (now the Pacific Ten), chairman of the search committee that brought President Norman Topping to USC, and chairman of the building committee that supervised the construction of the faculty center at the university.

Survivors are his wife Miriam and three children: John, of Rancho Palos Verdes, California;

Jane (Mrs. Roderick V. Smith), of Denver, Colorado; and Elizabeth (Mrs. Gary W. Larsen), of Long Beach, California.

RUSSELL CALDWELL
Professor Emeritus
University of Southern California

JAMES C. MALIN, emeritus professor of history and a member of the history faculty for forty-two years at the University of Kansas, died of a heart attack on January 26, 1979. He was eighty-five at the time of his death.

Born in Edgley, North Dakota, in 1893, Malin spent much of his youth in Edwards County, Kansas, but received the A.B. from Baker University in 1914. He obtained his M.A. in history from the University in Kansas in 1916 and the Ph.D. in 1921. Thereupon he joined the history department and served until his retirement in 1963; by that time he was one of the University's most distinguished scholars. Baker University conferred the L.L.D. degree upon him in 1962.

A president of the Agricultural History Society, of the Kansas State Historical Society, and of the Kansas History Teachers Association twice, Malin was also a member of the editorial board of the *Mississippi Valley Historical Review*, associate editor of the *Kansas Historical Quarterly*, fellow of the American Association for the Advancement of Science, and served on committees of the American Historical Association, the American Association of Geographers, and the American Studies Association. He profoundly influenced the careers of many students in history and other disciplines. Particularly was he effective with graduate students, and some, who were enrolled elsewhere, developed continuing relations with him. During the mid-1950s, several dozen promising young historians learned to respect him at first hand through attendance at a series of summer seminars at the University of Kansas.

Malin wrote some fifteen books, more than one hundred articles, and many book reviews. In the first decade of his career he published his doctoral dissertation, *Indian Policy and Westward Expansion* (1921) and two broad surveys: *The United States, 1865-1917: An Interpretation* (1924, 1926) and *The United States after the World War* (1930). These latter volumes were distinguished by their economic and social emphases and Malin's efforts to place national events in international and long-term perspective. During the early 1930s his interests focused upon local and regional history and he began path-breaking research upon agricultural and social adaptation to the subhumid environment of the North American grassland. He rapidly developed mastery of local history materials and the case-study method, but his research led him

also into geology, geography, ecology, climatology, and soil science, as well as anthropology and the history of technology. "The Turnover of Farm Population in Kansas," (*Kansas Historical Quarterly* [1935]) revised assumptions about the relation of climate to geographic mobility and employed methods that would be imitated by many later historians. Numerous other articles flowed from this research and the books, *Winter Wheat in the Golden Belt of Kansas: A Study in Adaptation to Subhumid Geographical Environment* (1944); *The Grassland of North America: Prolegomena to its History* (1947, 1956, 1961, and 1967); and *Grassland Historical Studies: Natural Resources Utilization in a Background of Science and Technology*, volume 1 (1950). Regional adaptation was the work primarily, he believed, of cumulative innovation and selection by many "common" men. The mastery of science displayed in these publications won him a considerable audience among scientists.

Throughout his career, Malin was keenly interested in the political struggles of the 1850s. The discovery of the lost territorial court records, and the availability of our relevant manuscripts, led him to reconsider John Brown's role in Kansas. *John Brown and the Legend of '56* (1942) was a tour de force in the analysis of evidence and the demolition of historical myth. Later, he turned to the Kansas-Nebraska Act, which had been of consuming interest to his teacher, Frank Heywood Hodder. *The Nebraska Question, 1852-1854* (1953) presented new evidence on the motivations of Stephen A. Douglas and reassessed the setting and the technological and political imperatives underlying the fateful statute.

During the 1940s, historiography and the philosophy and methods of history became increasingly of interest to Malin. *Essays in Historiography* (1946) dealt in part with the closed-space implications of Turner's thesis and its relevance for grassland history as well as with N. S. Shaler's contributions. The *Prolegomena* critiqued the work of Webb and others who had pioneered "toward grassland regionalism." In *Essays, On the Nature of History* (1954), *The Contriving Brain and Skilful Hand* (1955), and various papers, Malin also confronted theoretical and methodological issues. He rejected relativism and historical uncertainty as relativists defined it; relativist history, he maintained, was the handmaiden of totalitarianism. He explored the problem of historical certainty and the usefulness of other historical approaches, preferring to define the historian as analyst of the unique event within an open system. History, like ecology, was "the study of organisms in all their relations, living together, the differences between plant, animal, and human ecology or history being primarily a matter of emphasis."

Malin pursued his research after retirement, publishing articles and collections of essays dealing particularly with political reform and cultural and social life in Kansas. *Doctors, Devils and the Women: Fort Scott, Kansas 1870-1890* (1975) in part assessed the career of Dr. Sarah C. Hall; the interests of the profession had not outstripped him.

His findings on mobility, on dust storms and soil-building were controversial, as were Malin's rejection of ideas of Turner and Webb and the forcefulness of his views on relativism. Although essentially shy and gentle, he was courageously tenacious in defense of his ideas. Concerned primarily with historical substance, Malin was impatient with the formalism of referees and editors and, after the mid 1940s, he published most of his own books. If professional disapproval and his methods of publication restricted Malin's influence somewhat, his work inspired complimentary review essays, became the topic of a doctoral dissertation, and was the subject of a session before the Western History Association in 1972. His students and others, who have given his writings close attention, believe that his work will be judged of seminal importance by our successors.

A memorial fund to honor James C. Malin has been established with the University of Kansas Endowment Association.

ALLAN G. BOGUE
University of Wisconsin,
Madison

ALLEN M. WAKSTEIN, associate professor of American history at Boston College, died suddenly on January 4, 1979 in San Francisco at the age of 47. A product of the Boston Public schools, he took his B.A. from the University of Massachusetts and earned his Ph.D. at the University of Illinois. His dissertation, a study of the open shop movement during the 1920s, was directed by Fred Shannon.

After two years' teaching at Ithaca College, he returned to his native Bay State to assume a position at Boston College in the early 1960s. There, Wakstein was attracted to the then relatively unexplored field of urban history. With little material to work with and few signposts to guide him, he fashioned a course that placed this country's development within an urban context. Always concerned with the methodological demands of the historical profession, Wakstein compiled a collection of readings, *The Urbanization of America* (1970), that laid as much stress on approach as it did on factual content. The anthology was among the first in the urban field and nearly a decade later the questions it posed and the issues it addressed still retain their freshness.

Wakstein found tremendous pleasure in teaching, and with his history of Boston course, in the mid-1970s, he provided an instrument not only to

educate undergraduates, but also to reach out and acquaint elementary and secondary school teachers with the vast, untapped pedagogical wealth that lay at their disposal in the metropolitan environment. Through slide presentations and tours of neighborhoods, he used the physical city to bring the past alive and to make the present more understandable. His *Boston's History: A Resource Book* (1978), intended for both the classroom scholar and the intrepid urban explorer, reflected his belief that "it is the variety of artifacts that gives the teaching about [local history] an advantage." Less than a week before his death, Allen Wakstein chaired an AHA convention panel on "Teaching the Urban Experience: Boston as a Case Study."

For the past few years he was engaged in researching a full-scale history of the town of Framingham. Located twenty miles west of downtown Boston, Framingham (the community Al called home) was, as Wakstein liked to insist, neither a city nor a suburb, but an "urban place." He had become intrigued by the possibilities of community history, had offered a new graduate colloquium on the subject in the fall of 1978, and was scheduled to participate in the Newberry Library seminar to be held in March of 1979. He had been looking forward to the Newberry session with undisguised eagerness, for like all good teachers, Wakstein treasured the opportunity when he too could learn from others.

It was this openness of spirit that endeared him to his students and colleagues. His classes were popular not because he was a sparkling lecturer or witty story-teller, but because he took both his material and his students seriously. In years marked by sharp divisions on college campuses, his students realized that here was a person who really cared about their intellectual and social development. Colleagues, too, came to know him as a man of deep scholarly involvement. In addition to his book, Wakstein had published articles in the *Journal of American History*, *The Journal of the American Institute of Planners*, and *Labor History*. He had directed numerous masters and doctoral dissertations, and was held in high esteem by colleagues whose work he read. He was always available to offer critical, but constructive, comments on another's work, and he actively participated in the functioning of the department, as well as the university community.

He was a special man to all who met him, with a love of life and of his discipline. He brought the highest standards to the profession, and the Boston College community is a better place because of his devotion and activities.

JOHN L. HEINEMAN
Boston College

ROBERT WHEELER died suddenly on December 23, 1977, a month before his thirty-eighth birthday, of

a brain tumor. Bob was a vigorous, challenging, and stimulating friend and colleague whose energy and imagination touched everyone who knew him. Because he did so much in such a brief life span, his loss to the profession is as serious as it is to those of us who were close to him personally.

We met Bob at Stanford University in the fall of 1967 where the three of us had been hired to teach in the Western Civilization program. He had just come from the University of Pittsburgh where he was working on his doctorate in modern German history under Richard Hunt. We worked closely together there as part of a staff of twenty-one historians from all fields, during a time of great social as well as intellectual ferment at Stanford. In 1970 Bob left for the University of Southern California where he remained until the time of his death.

His work was primarily in the field of modern German social and political history. He acquired a fluency in German which permitted him to publish in that language as well as in English. His major published work was *USPD und Internationale* (1975). A related volume, *War, Revolution and Proletarian Solidarity: Grass Roots Internationalism in Germany, 1914-22*, is being prepared for posthumous publication by the University of California Press. In addition, he edited a volume of Curt Geyer's memoirs, and published over a dozen major articles in some of the most important historical journals on both sides of the Atlantic.

His interests expanded to various aspects of labor and social history. Recent papers at conferences and published articles were concerned with social structure and labor politics in Weimar Germany, labor history and quantification, and the interrelationship between sport and working class culture. He had methodically studied the archival collections in both the Federal Republic of Germany and the DDR on numerous occasions, and was the recipient of many fellowship awards, including grants from the ACLS, Fulbright Commission, American Philosophical Society, the Historical Commission of Berlin and the Humboldt-Stiftung Fellowship. He offered courses on a variety of subjects at the University of Southern California; one of the most innovative was "Film and Social Reality in Modern Germany" in which he utilized the German cinema as historical evidence.

He was the founder of the *Newsletter for the Study of European Working Class History*. The first issue appeared in 1972, and the list of library and individual subscribers rapidly mounted. In 1975 the name was changed to *International Labor and Working Class History*, and continues to thrive under the new editorship of Professor David Montgomery at the University of Pittsburgh. The *Newsletter*, and the

Study Group which supported it, was a product of Bob's vision, dedication, and resourcefulness. He stimulated contributions from many scholars, both established and younger specialists, on subjects usually left untouched by the regular journals of history—reviews of conference sessions on labor and social history, archival resources at institutions in Europe and America, and surveys of work-in-progress by scholars. The *Newsletter* remains a permanent monument to his selfless dedication to the study of history, his commitment to expanding the frontiers of the profession, and his desire to provide a means through which a community of historians would be able to communicate their ideas to one another more effectively.

In the past few years, he was especially proud of his involvement in sport history and sport as a way of looking at social history. He developed a very successful course at USC on "Sport and Modern Society" and delivered a number of papers at professional meetings on the subject. He published an article in the *Journal of Contemporary History*, "Organized Sport and Organized Labour," and was working on a larger study of the relationship between working class culture and sports. This new direction in his work was consistent with his often expressed beliefs that historians of working class movements had an obligation to deal with activities that meant something to their subjects. He was adamant in his sentiment that teachers of history could discuss events that were within the experience of their students without pandering to a false sense of "relevance." Bob's distaste for what he saw as academic snobbery was matched by the high academic standard at which his course on sports was taught.

Anyone who came in contact with Bob will remember something about him that was much more important than the courses he taught or the books and articles he published: the way he helped scholars and students and the intensity with which he approached his work, his professional colleagues, and his friends. Those of us who taught and worked with him will most of all think of a man who combined enthusiasm with integrity, and made no attempt to disguise either of them.

Bob was a warm and loving person. He leaves behind his wife, who knows all these things that he did, and his two children, who will learn of them later when they are older. We know that we are speaking for the many historians who knew Bob Wheeler, or knew of him, in expressing the grief and absurdity of his irreplaceable loss.

MARTIN A. MILLER
Duke University

CHARLES P. KORR
University of Missouri,
St. Louis

American Historical Association

Founded in 1884. Chartered by Congress in 1889
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MEMBERSHIP: Persons interested in historical studies, whether professionally or otherwise, are invited to membership. The present membership is about 14,500. Members elect the officers by ballot.

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PUBLICATIONS AND SERVICES: The *American Historical Review* is published five times a year and sent to all members. It is available by subscription to institutions. The Association also publishes its *Annual Report*, the *AHA Newsletter*, a variety of pamphlets on historical subjects, the bibliographic series *Writings on American History*, and *Recently Published Articles*. To promote history and assist historians, the Association offers other services, including an Institutional Services Program and the quarterly publication of the *Employment Information Bulletin*. It also maintains close relations with international, specialized, state, and local historical societies through conferences and correspondence.

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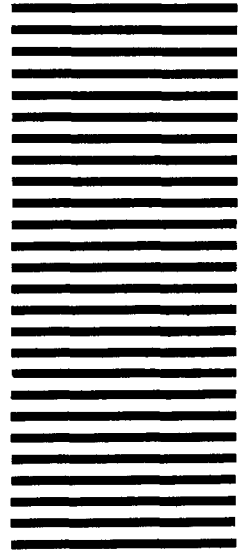
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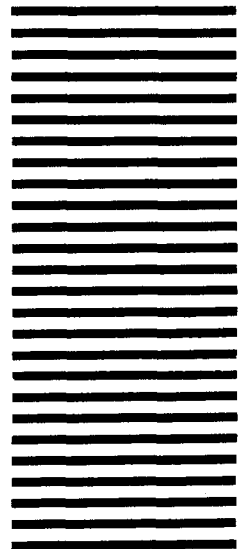
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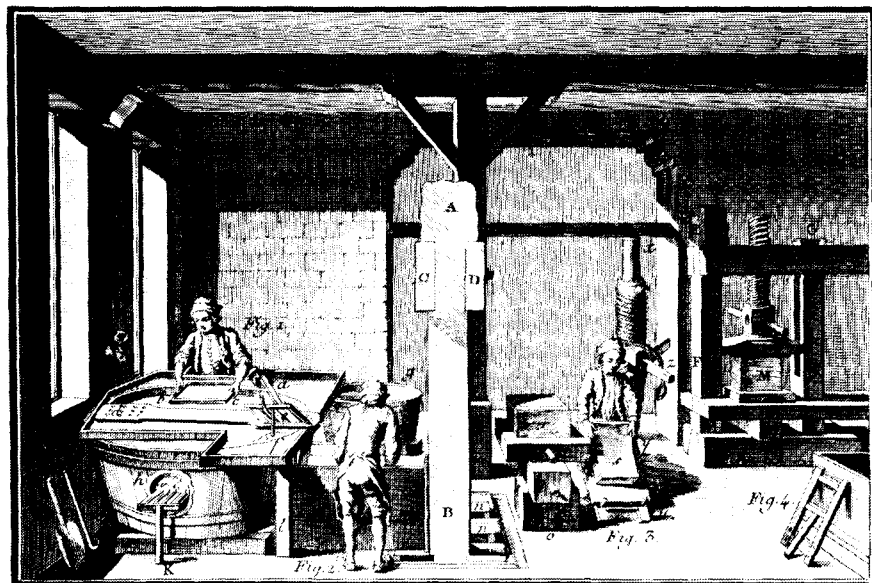


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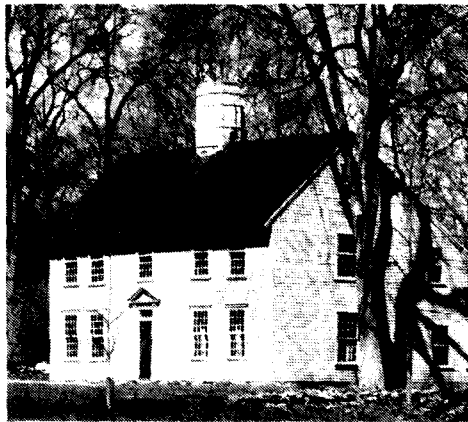
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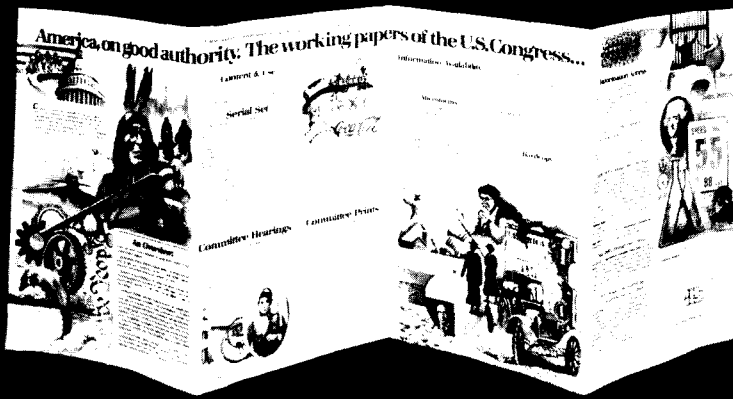
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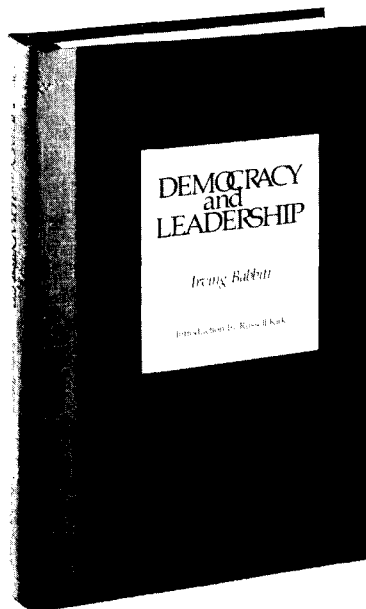
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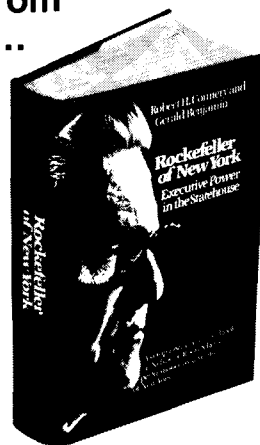
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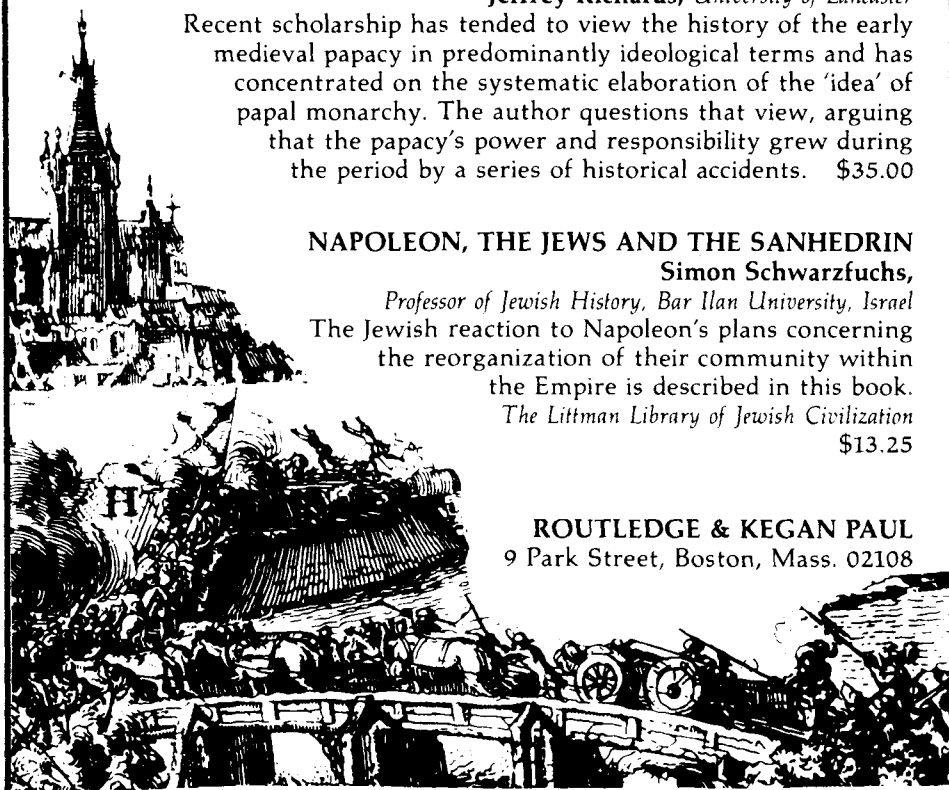
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